Psychological Bulletin

EDITED BY

JOHN A. McGEOCH, State University of Iowa

VOLUME 38, 1941

PUBLISHED MONTHLY (EXCEPT AUGUST AND SEPTEMBER)

BY THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

gm

Wahr

Vol. 38, No. 1 January, 1941

Psychological Bulletin

THE EXPERIMENTAL EMBRYOLOGY OF MIND[1]

BY LEONARD CARMICHAEL
Tufts College

I. INTRODUCTION

A human spermatozoan is a tiny, free, human organism. Its behavior is determined by the manner in which its living protoplasmic system reacts to the chemical, mechanical, and other energy relationships of its environment. It may be said that the movements of the sperm are chemotactic or rheotactic. When such a proper motile and mechanistically determined sperm fertilizes a proper human ovum in a suitable environment, processes start which sometimes lead to the development of a new and infinitely complex, but again freely moving, human organism. Certain of these organisms, so developed, at definite stages in their lives perceive, have emotions, reason, talk, build airplanes and tanks, and bomb other similarly constructed human organisms.

How do these big children of the human ovum and sperm come to have the mental capacities of adult men and women? In the growth from the fertilized immobile egg to voting citizen is there a point at which the new organism throws off the old mechanistic dependence upon the environment which held the movements of its father, the sperm, in rigid grip? This is a problem which has long been recognized, but which until recently has been answered mainly in terms of prescientific verbal speculation.

This paper, then, is devoted to a consideration of something of the present status of what used to be called the "original nature of man." In this treatment especial attention will be given to the early growth of the mind and to some guesses concerning the relationship of a study such as this to psychology in general. The functional study of the early growth of the animal body has been

[1] Presidential address delivered at the Forty-eighth Annual Meeting of the American Psychological Association, State College, Pennsylvania, September 6, 1940.

termed experimental embryology. By analogy, I have chosen to discuss in this paper the experimental study of the early development of those processes which are fundamental to man's mental life. This field I shall call *the experimental embryology of mind.*

It is fashionable, and wisely so, to present the presuppositions which are accepted before beginning the discussion of a fundamental psychological problem. It may be well, therefore, to say that in this paper it is taken for granted that, if any two living systems were ever absolutely identical in every physical and chemical respect, they would behave in exactly the same way under identical conditions.

Adherence to this mechanistic position makes it appropriate at once to ask certain other fundamental questions, if one is to study the growth or disintegration of the living organism. In the first place, what has been called the error of potentiality must be recognized and, save at times as a verbal convenience, avoided. This means that, scientifically speaking, we must not say that there is an oak tree in each acorn. The tree is not there, and poetic falsehood is not science.

To say, for example, that the cells of the developing optic cup are being formed so that the organism may see roses in its later life is to confuse what Aristotle called final cause with material or efficient cause. In other words, in our consideration of growth it is assumed that the only necessary conditions of phenomena are those events which precede in time the stage of development under consideration. In the growing organism it must be emphasized, however, that the whole past of the race as represented in the chromosomal and general germinal basis of heredity must be included in the temporal past.

In 1754 Étienne Bonnot, Abbé de Condillac, adopted in his treatise on sensation the artifice of the supposition of a statue with marble exterior whose sense organs are awakened from their sleep successively (**21**). Condillac, in warning his readers against what has just been called the error of potentiality, says:

> I wish the reader to notice particularly that it is most important for him to be himself in imagination exactly in the place of the statue we are going to observe. He must enter into its life, begin where it begins, have but one single sense when it has only one, acquire only the ideas which it acquires, contract only the habits which it contracts: in a word, he must fancy himself to become just what the statue is. The statue will judge things as we do only when it has all our senses and all our experience, and we can only judge as it judges when we suppose ourselves deprived of all that is wanting in it.

Let us follow the good Abbé's admonitions, avoid the error of potentiality, and look at the immobile mammalian fetus before its moment of animation or quickening. Let us then see how its behavior changes as its nerves and muscles develop and as its senses gradually become able, when properly stimulated, to initiate changes in the activity of the whole living protoplasmic system which we call an organism.

II. The Zero of Behavior

First of all it is important to remember that, at all stages, the fertilized germ cell, the embryo, and the fetus are alive and existing in an external environment. The dynamics of cell division, cell differentiation, and cell migration, and the dominance of certain cell groups over other cell groups occur and can only occur in this living, growing organism when the external environment has fixed limits of variability. That is, an environment which is organism-maintaining must have quite specific chemical, thermal, pressure, and other characteristics. Internal to the growing organism there are also fluctuating electrical, chemical, and mechanical relationships between cell groups which are basic in the growth of tissues and organs. Some areas of each complex growing individual have relatively rapid metabolism, others relatively slow metabolism. Moreover, in certain regions of the growing animal rhythmic cell movement begins early. In the human fetus, at about three weeks heart cells begin a rhythmic but faltering beat which will necessarily change and develop but still in some manner continue until that individual organism is legally pronounced dead (**55**).

Thus our statue, the fetus, is far from the dead marble of Condillac's imagination in the period before it first moves as a result of external stimulation. In its complex system of systems, an equilibrium is always being regained and in turn upset. In such alterations growth may be considered as a continuous shift of the baseline to which the restorations of the equilibrium of its life processes must ever return.

If we wish to place a zero point of mind at any level, in spite of the continuity of organic processes, it may be set at the first moment when the living system reacts to an external energy change so as to alter the external relationships of the system. Unfortunately, this zero point does not seem to come at the same place in the continuity of development in all organisms. A pretty and accurate picture has been painted of the early behavior of young fish embryos by Tracy (**51**). Without functional external recep-

tors these organisms are impelled to action by changes in their own internal environments. That is, by changes in the amount of oxygen available in the blood and by increase of metabolites in the blood, the central nervous system of such motile embryos is directly affected (**2**). Motor peripheral nerves as a result are activated, muscle movements take place, and acts of locomotion occur. These first responses are fully blind, but still by means of them the reorientation of the total organism in relation to its environment is accomplished. Such activity may well move the animal so that once again a proper supply of oxygen in the surrounding water is available for its needs. Gradually, in such fish embryos, the senses become functional, and the external control of behavior emerges, or, rather, here the external world through the external senses captures a precarious dictatorship of the organism. Such a Schopenhauer-like picture of the primary growth of the pure motor response and the subsequent development of control by the external world seems established in the fish embryo. As the investigator himself puts it: "The animal is driven through its environment as a result of stimuli which arise periodically in connection with its own metabolic processes." Thus we see a whole organism beating, as it were, in its outer environment as previously its heart cells beat in their own cellular environment.

Unfortunately for the esthetics of science, this pretty sequence cannot be found in overt form in all species, but the picture which it presents is instructive. Organisms differ from species to species in regard to the temporal sequence of functional development of the elements of the response mechanism. Muscles may be directly stimulated to response before neural control is established. Those who like analogies may look back from embryology to the sequence of phylogeny as proposed by Parker (**42**). According to this investigator, the independent effector or primitive muscle is the first specialized element of what is eventually to become the mammalian response mechanism. Only later in phylogeny do specialized receptors and nervous system develop and assume control of the more primitive contractile systems. The immature muscles even of mammalian fetuses act as independent effectors before neural control is established. Minkowski, for example, has shown that the first reaction of the human fetus to stimulation on the sole of the foot is ideomuscular—that is, a direct muscle response (**37**).

The fact that muscles can thus be directly stimulated has made

it difficult in certain instances to determine the exact time of onset of true externally excited behavior. Windle and Griffin describe a twitch of the forelimb of the cat fetus brought about by stimulation as a true reflex—that is, as a response involving external receptor, central nervous system, and muscle action (**57**). Coghill and his associates, on the other hand, possibly because their work has been done so largely in the amphibian field, believe that a response of the type described by Windle may well be an independent muscular response (**23**, **24**). In regard to this specific point, however, it may be said that Barcroft and his associates, working on the large fetus of the sheep, have confirmed the fact that the first response of this animal is of the nature of a neurally mediated reflex act (**3**). Because of the significance of this problem for all later investigations of the embryology of behavior, Bridgman and I have studied a large number of fetal guinea pigs, carefully dating the gestation period so that we would have litters at about the hours of first motility (**5**). We made moving pictures of the initial response of many of these fetuses. On the basis of this work we were able to conclude, with some assurance, that, in the guinea pig at any rate, the first response that can be elicited as the result of stimulation is a relatively localized one involving the lateral flexion of the neck and the sometimes simultaneous extension and later flexion of a forelimb. By a series of controls we convinced ourselves that this movement was a true reflex-like reaction and not a direct muscle response. This conclusion seems to have somewhat wide implications.

Coghill's painstaking studies of amphibian larvae made it possible for him to make a generalization concerning the early development of behavior in amblystoma which has been called the *Coghillian Sequence* (**22**). Briefly, this sequence shows that in amblystoma the first response is an ideomuscular one and the second a response of the total organism with a lateral flexion forming a C or reverse C. In the next stage it is noted that before the first flexion has been completed a new one on the other side begins. Thus the S or sigmoid movement develops. As this movement becomes more rapid it produces locomotion and is called swimming. In the salamander all this takes place before the limb buds develop. As the limbs grow, this sigmoid movement is well developed and so it is basic to the timing of the at first passive movements of the appendages. Thus the sigmoid movement is fundamental in the development of the walking, as well as the swimming,

rhythms. It should be noted, however, that the first movement of amblystoma occurs, unlike the condition found in the mammalian and human fetus, before the limb buds have developed. Hence Bridgman's and my observation that the first response of the fetal guinea pig is reflex in character does not in any way contradict Coghill's findings on amblystoma. Our results do suggest, however, that the Coghillian formula probably cannot be applied in all its details to the guinea pig and to certain other mammals which we have studied. In these mammals the limbs are well developed before the first stimulus-aroused behavior takes place.

There is much further evidence on this matter to which reference cannot be made here. Again it seems that empirical findings mar the esthetically attractive picture of the universal formula which would suggest that all behavior begins with the total organism and that reflexes appear as individuations from this total mass response. Above all it must be remembered that interspecies comparisons in behavior are difficult to make without scientific error. In this connection the work of Kuo (**33**, **34**) and of Orr and Windle (**41**, **60**) on the chick embryo should especially be considered.

As yet the facts concerning the zero of receptor-controlled motility in man are not clear. It is difficult to secure human material for such studies. Minkowski (**38**), Hooker (**29**), and a few others have studied the responses of such early human fetuses as have been available to them. Unfortunately, in almost all this work the observers have been forced to deal with dying organisms. Ordinarily the human fetus can be experimented upon only after the interruption of its placental blood supply. This means that the study of the behavior of the excised fetal organism, while interesting and extremely significant, is probably not typical of the behavior of human fetal organisms under normal conditions. My own experiments on the cat, rat, and guinea pig fetus, for example, show that even momentary interruption of the placental blood supply alters behavior in a most striking way. This fact is demonstrated clearly in an experiment carried out by Jasper, Bridgman, and myself (**30**). We were able to demonstrate that the electrical activity of the late fetal brain as shown in a fetal electroencephalogram was almost instantaneously abolished by tying the umbilical cord of the fetus so as to interrupt the supply of freshly oxygenated blood passing to the fetus from the placenta, where gaseous interchange with the maternal blood stream takes place. Following the

tying of the cord, however, the general body responses and reflexes of the fetus continued for some time in an unabated, and indeed in what seemed to be a definitely augmented, form. This observation merely confirms in a somewhat different way a whole series of quantitative studies by Barcroft and other investigators on the importance of a constant oxygen tension in the internal environment if behavior is to be studied comparatively (**2**). Therefore, in the case of the human excised fetus it is not possible to say with assurance that the first response is either reflex in character or one involving the total organism. All evidence considered, however, it seems to me that the first response of the human fetus is probably one involving a limb or other segment of the organism. Certainly, I am not convinced that the first human response is a so-called total body movement.

III. The Senses in Fetal Life

As already pointed out, the life processes of the fetus depend upon its own cell systems and upon the external environment in which it is growing. When the receptor systems become functional they mediate, as in the statue of Condillac, the special energy relationships between the fetus and its world. The ordinary physiological and psychological methods used in the study of the receptors cannot be applied in the study of the senses during the prenatal period. Methods which demand a verbal or symbolic response on the part of a coöperating human subject obviously cannot be used on a fetus. The employment of conditioned-response techniques and the use of various types of discrimination apparatus which involves learning are, in general, ruled out. So far only one general method has yielded satisfactory results in the study of the senses in fetal life. This method depends upon the initiation or alteration of receptor-induced effector responses which involve the so-called inborn reflexes of the organism. Motion-picture recording of such responses has been found to be a valuable aid to the accuracy of such reports. The use of the techniques of the electrophysiologist has also shown something directly about the growth of functional effectiveness in the receptors themselves and in the neural mechanisms associated with receptors. Electrical techniques have also been useful in recording certain temporal and and other characteristics of the muscle movements elicited by stimulation.

In this paper I shall not attempt to bring together in detail all

that is known about the senses in fetal life; especially, the mass of material summarized by Preyer (**45**), the great pioneer in this field, will not be referred to. I have tried to evaluate this material, including recent additions, in previous publications (**10, 15**). Here I shall, rather, attempt to limit myself to a description, and especially an evaluation, of some of the work on the fetal senses with which I have firsthand acquaintance.

IV. Cutaneous Pressure

Because it appears so early, the first sense field that we shall consider is the cutaneous-kinesthetic complex. Coronios (**25**), assisted in certain parts of his experiments by Schlosberg and by me, showed in a large series of litters of fetal cats of accurately known insemination age, that at about the twenty-third day after insemination (the normal gestation term of the fetal cat is 62 days) bending in the cervical region and foreleg flexion can be observed following cutaneous stimulation. Almost certainly such stimulation at this early age also involves kinesthetic receptors. In the work of Coronios, stimulation was ordinarily given by means of light brushes and blunt glass rods. Verbal records of behavior description were dictated at the time and moving pictures taken. As a result of these studies, Coronios showed, possibly more clearly than had been demonstrated before, that there is a continuous development of the behavior released by stimulation of the cutaneous receptors during the whole fetal period. From the first observed twitch there is an unbroken continuum of growth in the effectiveness and in the complexity of elicited responses in the fetal kitten. Windle, Griffin, and others have also made a very careful and illuminating study of the growth of behavior in the cat (**57, 59**).

To summarize the condition in the cat: At first, in response to tactile stimulation there are mere twitches; just before birth the responses of the fetus are coördinated and adaptive. Reactions that we call swimming, crawling, and defensive movements are easily elicited in the late fetal period. Such responses are as purposeful as any behavior which is unreinforced by language. In these experiments, as in the others with which I have been associated, placental circulation is maintained in each fetus during experimentation. The observations on the fetus are made under a bath of physiological saline solution held at blood temperature by an outer water bath which is thermostatically held constant by heating units.

Raney (**46**) and Lincoln (**36**), also working in the Brown University Laboratory, repeated on the rat fetus some of the work just described as done on the cat. The development of behavior in the rat fetus has also been studied by Swenson (**50**), by Angulo y González (**1**), and by Windle and his associates (**58**).

I have carried out a prolonged series of studies on the development of behavior released by all the senses during the fetal period in the guinea pig. In this work through the years I have been assisted by a number of colleagues and especially by my wife. More than 100 cutaneous pressure areas or reflexogenous zones have been investigated in a general study of the fetal guinea pig during its entire active prenatal life of 68 days.

The importance of quantified stimulation in such studies cannot be overemphasized. Smith and the writer (**18**), using a series of especially prepared Von Frey esthesiometers, were able to show the effect of different pressure stimuli upon the behavior of the fetal guinea pig at typical periods of development. *This study made it clear that from the first, stimuli, just at the threshold, tend to bring out responses which are characteristic of the point stimulated, but that more intense stimuli lead to a wider spread of responses sometimes involving almost all the muscle groups of the organism.* There is also a quantitative relationship between the intensity of the stimulus and the magnitude of elicited movement (**14**).

In regard to tactual stimulation in the fetal period in various animals the following conclusions may be drawn: (1) The place stimulated is most important in determining the character of the specific response elicited. (2) The age of the fetus influences the generality or specificity of response to some extent, but, if other factors are controlled, it seems that each reflexogenous zone has, as it were, a preferred reflex or pattern of response which is released whenever that zone is lightly stimulated, and this pattern remains very constant from the time it first appears to adult life. (3) The intensity of the stimulus is important both in the speed of the elicited response and in the spread of the response to narrow or wide motor outlets.

V. Temperature Senses

Lehner and the writer (**17**) studied the behavior released by thermal stimuli in the fetal guinea pig in a situation comparable to that just described for pressure stimuli. We arranged an apparatus in which there was a series of vacuum flasks containing liquid of known and graded temperatures. Each flask contained a

pipette. By stimulating the animal with drops of liquid at the temperature of its own body, a control for the tactual component of the drops of liquid was instituted. Using this procedure we were able to demonstrate that, as we moved to temperatures below the physiological zero of the organism or above the physiological zero of the organism, the intensity and the spread of the resulting response increased. The fundamental relationships of specific reflexogenous zones and specific forms of behavior were borne out in this study. Here again a quantitative relationship between intensity of stimulus and magnitude of movement was established.

VI. Cutaneous Pain

Surprisingly enough, our investigations show, as do certain previous observations by others, that a sharp needle is in general no more effective a stimulus, so far as the eliciting of behavior is concerned, in the fetal period than is the application of a blunt pressure stimulus such as a hair. The epicritic-protopathic theory of cutaneous receptors suggests that, in evolution, pain is more primitive than light pressure. The experiments which we have carried out give no support to this theory in ontogeny.

VII. The Proprioceptive Complex

It is difficult to isolate muscle, joint, and tendon receptor fields in the fetus. Coghill has said in the case of amblystoma: "The limb is able to respond very precisely to stimuli arising within the body (proprioceptive) as the result of a particular posture before it can respond to stimuli that arise exclusively from the outside world (exteroceptive)" (**22**). This observation is probably true in mammals as well. Certainly there is no sure evidence that this state of affairs does not also exist in the fetal cat, guinea pig, or rat. I have never, however, been able to eliminate to my satisfaction cutaneous components from kinesthetic stimulation in the early period of fetal life in the types which I have studied. I have attempted to elicit the tendon reflex in the fetal guinea pig, but without sure success. It is reported by Minkowski (**38**), however, that such reflexes may be elicited in the human fetus. The so-called *Magnus Reflexes*, the tonic postural changes of the organism brought about by the bending of the head in relation to the trunk, and so forth, can to some extent be elicited in the fetal guinea pig. Such responses are more adequately elicited in late fetal organisms in which the higher brain centers have been operatively removed.

VIII. The Nonauditory Labyrinth

Righting responses in the case of organisms which are in contact with a pressure surface can be observed in prematurely delivered fetal cats and fetal guinea pigs, as well as in other mammals. The maintenance of the upright posture when supported only by the liquid in the experimental bath is also observable in these fetuses. But this cannot be attributed to the nonauditory labyrinth alone. The extent to which such responses are tactually and kinesthetically determined is not known. In the same way, the part played by vision in such postural righting is not easy to determine at certain late fetal periods in the guinea pig. The tension of the umbilical cord also may play a part in these responses during the period of free swimming in the bath of warm saline solution.

It is possible to study the genetic development of the air-righting reflex by high-speed motion-picture photographs (**40**, **56**). Keller and I have done this in newborn kittens (**11**). Warkentin and I have repeated this work on kittens, rabbits, and guinea pigs (**53**). As a result of these studies there can be no doubt that the guinea pig is born with a relatively high capacity to right itself in the air when falling and thus to land on all four feet no matter from what position it is dropped. Interestingly enough, this capacity is by no means absent in prematurely delivered guinea pigs several days before normal birth. Hence, by inference it may be said that in the fetal stage the requisite mechanism for this response has sufficiently matured to make it function effectively. The situation is quite otherwise, however, in the cat and rabbit. A newborn cat falls through the air without any tendency to right. The maturation of this capacity, however, occurs rapidly during the early postnatal period. It may be interesting to note that this response begins, in all animals studied, in the head region, and duplicates to some extent in its gradual perfection the sequence of responses by means of which the air-righting reflex is brought into operation each time it occurs in any adult mammal. As I suggested some years ago, the shortening of the time sequence in the perfection of this act may be a developmental principle of wide general significance. Experimental controls instituted by Warkentin in the study of this reflex make it seem almost certain that the growth in the capacity to air-right is not at all a result of learning or conditioning. It is a function of the gradual inner growth or maturation of the receptor-neuromuscular mechanisms involved. This maturation occurs at different rates in different species of animals

and also in different individuals of the same species, but we have no evidence that practice is important in its perfection.

IX. The Organic Senses

Little experimental work has been done upon the organic senses in any fetus, although studies have been carried out on the movement of fetal intestines (**4**, **61**, **62**). Pfaffmann has been able to show that in prematurely delivered cats and in newborn kittens and guinea pigs the sucking of milk is a function to some extent of the fullness or emptiness of the stomach (**43**).

X. Smell

No satisfactory experimental work has been done on the sense of smell in any fetus. I have attempted a few casual experiments in this connection, but none of them is worthy of description here. In the human prematurely delivered infant there is some evidence that the individual may avoid a breast covered with kerosene but take one lubricated with odorless oil of the same viscosity (**44**).

XI. Taste

Pfaffmann devised an apparatus by means of which it is possible to record graphically the sucking reactions of a newborn kitten (**43**). By a system of stopcocks it is possible to alter the character of the milk being sucked. Thus taste stimuli may be introduced at known times. If an alteration appears in the sucking curve at the time the new substance is introduced, it can reasonably be assumed that the receptor mechanisms have been activated as a result of the novel stimulus. The conclusion of this study indicates that there is a distinction between sweet on the one hand and sour, bitter, and salt on the other. No sure distinction between the three classes of so-called noxious taste stimuli, however, has been secured in the experiments which have so far been carried out. These experiments are essentially of a preliminary nature.

XII. Audition

Kussmaul years ago said: "Of all the senses, that of hearing sleeps the deepest" (**35**). So far as fetal mammals are concerned, however, this is only relatively true. Rawdon-Smith, Wellman, and I (**47**) have been able to demonstrate by the use of suitable amplifiers and oscillographs that air-borne auditory stimuli are effective in eliciting the so-called *Wever-Bray Effect* or electrical

cochlear response on the same developmental day in which the first overt behavior to auditory stimuli is brought about in the fetus of the guinea pig. That is, in a fetus 16 days before normal birth-time a small electrical response of from one to two microvolts in peak can be obtained to an air-borne auditory stimulus of 600 cycles per second, at a relatively high intensity. Decreasingly great electrical output is obtained to tones below this and above 2000 cycles per second. The voltage of electrical potentials so elicited increases rapidly as fetal maturity progresses, so that by six days before the normal birth-time an electrical response of at least 100 microvolts can be recorded. This experiment gives a pretty illustration of the usefulness of electrical recording in connection with receptor activity itself.

XIII. Vision

Work by Coronios (**25**), Warkentin (**52**, **54**), and others with whom I have been associated has shown in several species of animals some of the facts of the onset of visual capacity during the fetal and neonatal periods. This is a difficult field in which to work. There is very great difference in the readiness of the visual mechanism to function at the time of birth in different species of mammals. The guinea pig's eyes are open, and light stimulation can be shown to be probably effective in the fetal period on the fifty-sixth post-insemination day. In the cat, on the contrary, as is well known, the eyes do not open until a number of days after birth, a period varying, in Warkentin's observations (**54**), from 3 to 15 days. The motor mechanisms about the eye also mature slowly, but they can sometimes be brought into play before visual stimulation is effective. Eyewinks can be elicited to tactual stimulation of the lids or cornea before such responses are brought about by photic stimuli. The first eyewink to be called out by tactile stimulation in the guinea pig fetus occurs in the still unopened lids at 35 post-insemination days (**12**). At this time there is also a slight change of the total eyeball in the orbit, apparently as a result of the contraction of all the external eye muscles. Evidence is also developing in experiments which Wellman and I are conducting at the present time which suggests that eye movements can be elicited by the proprioceptive stimulation caused by changing the body in space before they can be elicited by any form of light acting as a retinal stimulant. In some of this work we have availed ourselves of the well-known phenomenon that passing striations of

light over the eye of an organism may, when conditions are satisfactory, set up compulsory, reflex, optokinetic nystagmus. Using these inborn and highly integrated responses, it is possible to study the onset of certain aspects of visual capacity in the fetus in an exact and quantitative way. In order to record these fetal eye movements we have found, as reported in a still unpublished report, that the electrical recordings of shifts in the corneoretinal potential from the small eyes of unborn guinea pigs can be made with excellent results (**16**). Our preliminary conclusions indicate that, at least from 60 postinsemination days onward, such visually released nystagmus to moving striations can be elicited and recorded in the fetal guinea pig. Hence it is possible to say that vision of a measurable acuity develops before birth in this animal.

Using a similar technique but varying the striations used, Warkentin has made elaborate studies of the development of visual acuity in newborn and prematurely delivered cats, rabbits, guinea pigs, other mammals, reptiles, and amphibia (**52**). These results show in general that in ontogeny there is a gradual development of pattern vision—that is, the capacity of the eye to respond to sharp differential gradients of light. This change in the latter part of the fetal period and in the early part of the neonatal period is a function of a number of developmental changes in the optic media of the receptor organs and their associated neuromuscular mechanisms. The maturation of the retina, the optic nerve, and optic centers is important in the perfection of these responses. Incidentally, in this work some evidence suggests that the vitamin content of the diet is very important in determining the onset of acuity vision, and, since radiation is important in diet, care must be exercised in comparing the effectiveness of eyes of animals reared in the dark and those reared in the light. In certain experiments which allege that light stimulation and, inferentially, learning are important in the early development of effective vision, this factor has not been controlled. Mowrer, however, has shown that exercise is not without its effect in the development of this mechanism (**39**).

The technique described above has been adapted to studies of normal and premature human infants. Because of the intimate relationship between Vitamin A deficiency and night blindness, it has been found possible to use this technique as a sensitive diagnostic aid in connection with tests for Vitamin A deficiency in newborn babies.

In what has just been said about the development of the cutaneous, auditory, visual, and other senses it has been necessary by inference to refer to the growth of motor effectiveness. Now, however, we may, it seems, advantageously turn from our statue's senses to its behavior repertory itself.

XIV. Motor Development

Coronios, on the basis of the study of many litters of fetal cats mentioned above, was able to draw up a time schedule for the expected appearance of specific responses in that animal (**25**). In the comparable study of the fetal guinea pig over 100 areas and receptor fields were selected which were systematically stimulated in a series of fetal litters of known postinsemination age. In this study 96 different litters were used. Of course, it was not possible to stimulate each point on the youngest fetuses, but the whole series of zones, or as many of them as possible, was studied in each fetus used (**12**). Motion-picture records and dictated protocols were made of each experiment. At the conclusion of this particular experimental series, which occupied many months, the different protocols were recopied in relation to the area stimulated. Then for the first time what seems to me a remarkable fact emerged. In spite of some masking at times by general activity, there appears to be a *pattern of behavior which is the characteristic response of virtually every exteroceptive point stimulated on a fetus*. As noted before, when stimuli much above the lower threshold are used, this pattern is often masked by general behavior. Similarly, when other stimulation, as from the pressure of the supporting surface, acts on the fetus, other behavior than that typically elicited by the applied stimulus may take place. But in general each stimulus point or cutaneous reflexogenous zone, when optimally stimulated, releases a pattern of behavior which is remarkably constant from the first time it appears in early fetal motile life until birth and, indeed, until adult life. For example, when a stimulus is applied to the upper lip just to the right or left of the midline on the snout of a guinea pig fetus of 50 postinsemination days, very specific behavior results. In such cases the paw on the stimulated side is almost invariably brought to the point of stimulation. If the stimulus is moved a millimeter from one side of the midline to the other, the other paw is at once brought into play. This same reaction can be demonstrated with other types of stimuli such as drops of warm or cold water, but not necessarily by drops of water of

the same temperature as the fetus. Thus it is not the physical character of the stimulus, but rather that it shall be above the threshold of some of the complex of skin receptors and in a specific locus, that determines the response. Such typical patterns of behavior remain amazingly constant in an organism that is rapidly growing, and, conversely but similarly, growth may suddenly alter such responses, and such alterations of behavior may easily be confused with learned responses, especially in postnatal life.

This description may suggest what has been called behavioral atomism. If so, the fetus, not the investigator, may be blamed for the sin. The statement just made does not mean, however, that adult behavior or integrated behavior at any age is any mere tying together of discrete patterns of response. Of course, a "bundle hypothesis" in this sense is not a true description of the facts of growth. In the first place, adaptive responses are seldom elicited by a series of pressures given in special temporal orders, as it were, upon a series of cutaneous doorbell buttons. A chain of central processes and temporally discrete proprioceptively and exteroceptively aroused events is involved in such responses. Nevertheless, I have never seen any responses in the late fetus which, in their elements, have not appeared as a typical patterned reaction to isolated stimuli many times before. In the late guinea pig fetus the hair coat is well grown, the teeth are erupted, eyes and ears are functional, and adaptive integrated behavior is well established. At this time such an animal will, to use the language of teleology, attempt in a most effective and even ingenious way to deal with a tactual stimulus applied to its lip. First, it may be, it will attempt to remove the stimulus by curling the lip; then, if the stimulus remains, it is brushed by the forepaw on the stimulated side. If the stimulus still persists, the head is turned sharply. Finally, a general struggle is resorted to which involves movements of all four limbs and all trunk muscles. In a late fetus this final maneuver is sometimes so quick and effective that the experimenter is often thwarted and the offending stimulus is removed—by a guinea pig fetus that is having its own willful and annoying way in spite of anything the experimenter can do. Each of these special responses, however, may be seen as an old one to the person who has watched the growth of fetal behavior. Moreover, it does not seem to be an aid to understanding to say that such purposeful and effective behavior is the response of the organism as a whole.

Rather, it can be said that this behavior illustrates the fact that the course of fetal development is the story of the initial appearance and then the continued maintenance of a wide variety of specific mechanisms. The timing and interplay of these mechanisms make the admirable machine of the tiny body able to adapt itself in many varying and successful ways to environmental changes. Thus, mechanistically and, I believe, without environmentally determined learning these little machine-like organisms grow more effective as they become older. Complex patterns of behavior emerge as a result of maturation. Such behavior is possibly as truly end-seeking and purposeful as is any behavior in the world which does not involve the use of language. I see no reason to believe that this emergent purposeful behavior is not as natural a result of the processes of growth as is the length of the fetal whiskers, and quite as independent of learning.

The growing animal functions in a way that is in general adaptive at every stage. When I wrote my first papers in this field, dealing with the development of drugged amblystoma (**6**, **7**, **8**, **9**), I was so under the domination of a universal conditioned reflex theory of the development of adaptive responses that I denied categorically the truth of the statement just made. But every experiment that I have done in the field of the early growth of behavior has forced me to retreat from this environmentalist hypothesis. Now, literally almost nothing seems to me to be left of this hypothesis so far as the *very early* development of behavior is concerned.

XV. General Implications of the Study of Fetal Behavior for Psychology

Besides providing a genetic description of the growth of sensory-controlled behavior and behavior which external observers call purposeful, there are also other implications of the study of fetal behavior for psychology. Indeed, in general it may not be too much of an exaggeration to say that the experimental embryology of mind may provide a basis for the psychology of adult human mental processes which is comparable to the basis for adult anatomy provided by a knowledge of the morphological embryology of the human organ systems. For example, one of the oldest and most insistent puzzles concerning man, or, if you wish, our adult statue with all its senses and nerve centers, is that many stimuli which are very diverse as physical energies release behavior

which is identical, or nearly identical, in the living individual. Likewise, the energies of stimuli which are very similar or even alike in the quantitative descriptions of the physicist sometimes release patterns of response which are quite dissimilar in different organisms or in the same organism at different times.

How is deterministic experimental psychology to deal with this problem? It seems that other natural scientists sometimes look with suspicion upon all psychology because this embarrassing question exists, not alone because it is not answered to their liking. May psychology remind such scientific friends that because the living organism does not act as some simple machines do does not disprove a mechanistic approach to psychology? One can well imagine an old-fashioned mechanical engineer looking at a steam turbine and saying that the new device cannot be a steam engine since it possesses neither pistons nor cylinders. For such a man pistons and cylinders are the essence of steam engines. In a similar way even some psychologists seem to feel that, because of the facts just described concerning the biological and psychological equivalence or nonequivalence of physically identical stimuli, the organism cannot be subject to deterministic rules. Such individuals, it seems, are merely defining the word "mechanical" in a special way. To them it refers not to the basic principles of mechanics but rather, it may be because of a limited past experience, only to certain classes of machines—for example, those which have gears.

Parts of the answer to the question of how it is that dissimilar physical stimuli are similar or apparently identical in their effects on living organisms require, it seems to me, an appeal to the experimental embryology of mind. First let us look at the facts. In recent years Klüver's significant studies (31), and those of many other individuals, have focused experimental and theoretical attention upon this subject. Klüver has succeeded in showing experimentally, at the perceptual level, that physically heterogeneous situations are sometimes treated as identical by animals. This may be taken as demonstrating the existence at such times of basic behavior mechanisms in the organism which are fundamental to the acts observed. It is my belief that the complete story of the embryology of mind-as-behavior will give the ontogenetic history of such mechanisms, and thus it will become clear that the specific maturation of the receptors and the nervous system determines the identical responses that are made to different

types of stimuli as already described above in examples taken from fetal life.

In this connection the stability of patterns of response in fetal life takes on a new significance. The growing anatomical structures of the guinea pig fetus are so organized that a wide range of stimuli, if appropriately applied, call out essentially the same response throughout a long period of growth and, indeed, throughout life. The degree of suddenness of change in a gradient of stimulation affecting a receptor field, for example, may be more important than the absolute physical characteristics of the energies of the stimulus. In understanding fundamental behavior of this sort, too much emphasis cannot be placed upon the real advance in psychological thinking made by the results of the gradient experiments of Köhler (**32**). While his experiments have been largely limited to the perceptual fields, their implications are as wide as animal behavior.

It is interesting to note that a whole series of characteristics of stimulus patterns, as they are applied to the organism, such as suddenness of onset, cessation of application, movement, and, above all, change in intensity, initiates in receptors what may be called neural signals and little else that is important. These signals or propagated disturbances pass to the central nervous system. A sudden flash of light, a sudden sound, or an appropriately placed sudden tactual stimulus may all call out an eyewink in a late guinea pig fetus. These stimuli are physically quite heterogeneous, but they may be said to be, in one respect at least, equivalent. Only by empirical experimentation can such families of equivalence be determined. But possibly more important is the fact that only by a knowledge of the developmental history of specific behavior mechanisms can such reactions be made amenable to that sort of scientific description which allows prediction of future specific reactions. Suddenness of presentation, movement of the stimulus, and the like are, of course, those characteristics of stimulus pattern which classical psychology singled out to describe as the determiners of attention. It may be significant for adult psychology that these stimulus characteristics are already effective in fetal life.

To revert to the physiological level, it is important to notice that alteration in the make-up of the fluids constituting the internal environment of the central nervous system may sharply modify behavior. Some of the characteristics of behavior in late

fetal life, as contrasted with those of early fetal life, are almost certainly due to the fact that not only has the nervous system matured, but also the brain of the late fetus has less oxygen at its command than has that of the early fetus. This picture is a most complex one. The higher centers of the brain in the case of the late fetus are functionally more active than in the earlier period, and it is just these higher centers which are most subject to oxygen changes in the blood. In the same way internal alterations in temperature, blood sugar, water, and the like allow the external environment to call out behavior which would not be elicited in different internal conditions of the organism.

An interesting example of a late-maturing pattern of behavior which can be elicited by appropriate stimulation of specific reflexogenous zones ordinarily only after sexual maturity, and then only in the brief period of sexual receptivity, once each 16 days, is the copulatory reflex of the female guinea pig. This reflex, fully described a few years ago by Young (**64**), can be elicited in all normal female guinea pigs during a period of a few hours in length only once during each receptive period of the reproductive cycle. This very definite reflex can be called out by direct tactual stimulation of the reflexogenous zones on the animal's back and flank. This so-called reflex is, of course, in reality a complex behavioral act involving almost the entire musculature of the organism. As a patterned act it is unlike any other response of the guinea pig, although its elements are old. It appears in complete form the very first time the appropriate stimulus is applied, even in an animal which has been reared alone. Experimental hormonal manipulation of the blood stream of the guinea pig, moreover, will alter the character of this response. Here, then, we have a specific stimulus-neuromuscular pattern which appears only when the chemical make-up of the internal environment has certain very specific characteristics. This late response may then be said to be complicated, adaptive, and yet essentially independent of learning.

The work of Carpenter on the reproductive behavior of the pigeon likewise shows that the condition of the internal environment of the bird determines in a very specific way the repertory of complex patterns of behavior which is typical of each stage of reproduction (**19, 20**). Such responses may, therefore, without too much error, be called inborn. For example, when nest-building is the order of the day, any little sticks in the bird's visual field are selected with greatest care and piled into nests. As soon as the

cyclic chemical clock of the internal environment has passed what may be called the nest-building point, behavior changes. At this time the perceptual and attentional field of the bird alters. Twigs, straws, and other nest-building materials are no longer important; other responses related to egg-laying take their place. Experiments by Stone (**49**) and by Richter (**48**) show analogous relationships in the rat. The recent reports given by Yerkes (**63**) and by Crawford (**26**) on social dominance and the menstrual cycle in chimpanzees are significant in this connection.

Much of the infinite complexity of the adaptive behavior of the adult organism thus appears to be a result of timing and interrelationships of the patterns of response which are set off by the alterations of the external environment acting upon an organism which is always conditioned by the cyclic tides of an ever-changing internal environment. In this connection it is interesting to note that the afferent neural impulses initiated by the external senses play upon what is today seen clearly in all electroencephalograms to be a continuous rhythmic and changing background of activity in the central nervous system itself (**30**). In spite of this I am willing to follow Guthrie and Horton (**28**) when they point out that even in learned problem-solving behavior the activity of adult organisms may be at times as stereotyped as we have seen it to be in fetal life. Thus, given the same set of stimulus conditions, internal and external, the animal's posture, even of the tail, is stable in trial after trial. This evidence suggests that once a pattern of behavior has been established in fetal or later life by growth or by those physiological processes which underlie learning in later life or by growth and learning in coöperation, it will continue to be released without change in a similar stimulus situation. That is, it will so continue until definitely altered by changes in inner or outer conditions. It is clear that even certain language mechanisms of vocal and subvocal speech behave in this way.

This same point of view has interesting connotations for what is called the psychology of needs or drives in their relation to the experimental embryology of mind. The needs of the fetus are few. Oxygen, water, food, and an even thermal environment are provided and regulated by the maternal organism. If one clamps off the blood supply of the umbilical cord in late fetal periods, as noted above, an increase in activity on the part of the fetus is often observed. As scientists, we external observers know in such cases that the organism needs oxygen. In the fetus, however, it is

clear that the growth of the nervous system has provided a mechanism which now, the first time it is called upon, makes gasping and air-breathing possible. Here we have a simple and typical example of "need psychology" in a fundamental form. The physiologist knows that the organism must have oxygen if it is to survive. The nervous system of the fetus is so constructed that when its internal environment comes to lack oxygen, already existing neural patterns basic to patterns of behavior are released. These responses may supply this want by establishing air-breathing. The only proviso is that the neural and muscular mechanisms be ready. This means, then, that what the external observer correctly enough in this case calls the "organism's need" is an alteration in the internal environment of the fetal nervous system. As a result of this change, previously inoperative patterns of behavior are activated. In this example, therefore, the word "need" may be abandoned. It adds little to the understanding of this sequence of behavior. The responses that are observed to result from oxygen lack are released as are the other responses. In this typical case, changes in the internal environment are sufficient to allow the release of specific inborn neural patterns, which in turn set off specific adaptive patterns of behavior.

In the fetus and in the prematurely delivered guinea pig, other needs are less easy to demonstrate. Skin cooling, in certain instances, however, does lead to changes in heart rate and breathing rate and even to shivering and motor movement. These responses to thermal stimuli again may be considered as adaptive forms of behavior. They are released by upsetting what has been called the living balance of the organism. The so-called need for food in the prematurely delivered fetus can also be demonstrated. For many days before normal birth it is possible by appropriate stimulation of the fetal lips to bring about typical sucking reflexes, at least so far as the lips and tongue are concerned. This pattern of behavior in the human individual has been called *Thompson's Reflex.* Such responses can be elicited in the fetus long before there is any need for external alimentation on the part of the fetus. It is important to understand, however, that these responses can also be elicited after birth or premature delivery when food is biologically necessary for the survival of the individual. In these latter cases, if liquid is introduced into the mouth, it in turn releases the sucking reactions. The added stimulus of the liquid now sets off the inborn swallowing reflexes, and the ingestion of food from the external

world begins. When this course of activity fills the stomach or leads to alterations in the blood stream, the reflex pattern of sucking, as noted above, is altered. External stimuli which a little time before released the sucking reflex now do so no longer. We say that the animal is satiated. A new course of behavior is often elicited by the same stimulus which previously caused sucking. It may lead to head-turning and so-called avoidance responses.

Similar statements can be made in relation to the inborn or maturationally developed responses related to elimination, rest, change, and sex. Thus, as I see it, the contribution of the experimental embryology of behavior to an understanding of the psychology of need and motive is clear. It suggests that the mechanisms which are released by appropriate internal and external patterns of stimuli in so-called deficit or need states are not set apart from the rest of behavior. They are merely typical of all conditions in which fundamental inborn or learned mechanisms of the organism are brought into play as a result of specific forms of stimulation. The characteristic which has led to the elevation of *need states* as a special form of behavior is, of course, the fact that the stimulation in such cases is often dependent upon changing conditions inside the organism. Behavior so internally determined persists as the organism moves through an external environment made up, it may be, of very varying physical energies. The persistence of behavior associated with such desires, as they are called in traditional psychology, can be understood in terms of the embryology of behavior as a mere extension of the conditions which govern other and simpler types of stimulus-released behavior. This fact is especially important to consider in connection with theories of learning which set up *need-satisfying* forms of response as a special category or even explanation of learning. The facts presented here suggest, rather, that the full internal and external stimulus condition, *before* the successful act rather than after the act, is fundamental in giving a scientific basis or explanation of learning, as recently suggested by Guthrie (**27**). The view could also be defended that an understanding of the early growth of behavior will make it easier to understand certain other insistent problems of learning (**13**). The words "natural organization," "spontaneous association," and "pseudoconditioning" describe different forms of behavior of great importance. These phrases take on new meaning when viewed in terms of the gradual growth and inner development of the living response mechanism.

The last paragraphs have given mere hints of fields which seem to me not unrelated to the experimental embryology of mind. They have been introduced to indicate that the description of early individual behavior may eventually be seen to throw light upon some of the basic problems of scientific psychology. In this paper I have attempted to demonstrate that the fetus may be described as machine-like in the precision and repetition of many of its patterns of response in the same stimulus situations. I have also suggested that the complex behavior of the adult organism may in certain aspects be viewed in a similar manner. That is, the machine character of the fetus does not seem to be lost as development progresses, provided only it is recognized in its early stages when it can be most clearly seen. The machine, as well as what it can do, becomes more complex, but it does not become in any sense non-mechanical. This is still true, it seems to me, when the organism's behavior is of the sort that external observers call purposive, or when it is adaptively modified by learning.

Much that I have said in the latter part of this paper has obviously been speculation in advance of full experimental evidence, but I have not made any statements that have not seemed to be forced upon me as the result of my observations of the growth and change in the behavior of organisms before learning begins. I have attempted further to suggest—as little more than an article of belief, it may be—that the experimental embryology of mind is not unrelated to an understanding of adult behavior and adult mental life. Is it not possible that this same point of view will also be useful as we consider social behavior and abnormal human behavior such as that shown in states of extreme anxiety and frustration? Here indeed, as Hughlings Jackson long ago suggested, a knowledge of the evolution of behavior may provide a clue to an understanding of its dissolution.

The scientific psychologist, of course, has other and, it may be, much more basic approaches to an understanding of adult human mental life than that here called the experimental embryology of mind. The philosopher, the esthetician, the moralist, the exponent of religion, and the other students of human values will rightly insist upon the consideration of other and, from certain points of view, more fundamental approaches to the great aim of the explication of the human mind and spirit. In this paper I shall be more than satisfied if the fetuses and I have done one thing. We shall feel rewarded if what has here been called the experimental em-

bryology of mind can be seen to have some small, but real, contribution to make, even at the simplest level, to a paradoxical and impertinent enterprise—the enterprise which psychologists have taken as their own—the human mind's ambitious attempt to understand itself.

BIBLIOGRAPHY

1. Angulo y González, A. W. The prenatal development of behavior in the albino rat. *J. comp. Neurol.*, 1932, **55**, 395–442.
2. Barcroft, J. The brain and its environment. New Haven: Yale Univ. Press, 1938.
3. Barcroft, J., Barron, D. H., & Windle, W. F. Some observations on genesis of somatic movements in sheep embryos. *J. Physiol.*, 1936, **87**, 73–78.
4. Becker, R. F., Windle, W. F., Barth, E. E., & Schulz, M. D. Fetal swallowing, gastro-intestinal activity and defecation in amnio: an experimental roentgenological study in the guinea pig. *Surg. Gynec. Obstet.*, 1940, **70**, 603–614.
5. Bridgman, C. S., & Carmichael, L. An experimental study of the onset of behavior in the fetal guinea pig. *J. genet. Psychol.*, 1935, **47**, 247–267.
6. Carmichael, L. Heredity and environment: are they antithetical? *J. abnorm. soc. Psychol.*, 1925, **20**, 245–260.
7. Carmichael, L. The development of behavior in vertebrates experimentally removed from the influence of external stimulation. *Psychol. Rev.*, 1926, **33**, 51–58.
8. Carmichael, L. A further experimental study of the development of behavior. *Psychol. Rev.*, 1928, **35**, 253–260.
9. Carmichael, L. The experimental study of the development of behavior in vertebrates. *Proc. & Pap., 9th Int. Congr. Psychol.*, New Haven, 1929. Pp. 114–115.
10. Carmichael, L. Origin and prenatal growth of behavior. *In* Murchison, C. (Ed.), *A Handbook of Child Psychology.* (2nd rev. ed.) Worcester: Clark Univ. Press, 1933. Pp. 31–159.
11. Carmichael, L. The genetic development of the kitten's capacity to right itself in the air when falling. *J. genet. Psychol.*, 1934, **44**, 453–458.
12. Carmichael, L. An experimental study in the prenatal guinea pig of the origin and development of reflexes and patterns of behavior in relation to the stimulation of specific receptor areas during the period of active fetal life. *Genet. Psychol. Monogr.*, 1934, **16**, 337–491.
13. Carmichael, L. A re-evaluation of the concepts of maturation and learning as applied to the early development of behavior. *Psychol. Rev.*, 1936, **43**, 450–470.
14. Carmichael, L. Stimulus intensity as a determiner of the characteristics of behavior in the fetal guinea pig. (Abstract.) *Science*, 1937, **86**, 409.
15. Carmichael, L. Fetal behavior and developmental psychology. *Onzième Congr. int. Psychol.* (*Rapp. et C. R.*), Paris, 1938. Pp. 108–123.
16. Carmichael, L. A technique for the electrical recording of eye movements in adult and fetal guinea pigs. (Title only.) *Psychol. Bull.*, 1940, **37**, 563.

17. CARMICHAEL, L., & LEHNER, G. F. J. The development of temperature sensitivity. *J. genet. Psychol.*, 1937, **50**, 217–227.

18. CARMICHAEL, L., & SMITH, M. F. Quantified pressure stimulation and the specificity and generality of response in fetal life. *J. genet. Psychol.*, 1939, **54**, 425–434.

19. CARPENTER, C. R. Psychobiological studies of social behavior in Aves. I. The effect of complete and incomplete gonadectomy on the primary sexual activity of the male pigeon. *J. comp. Psychol.*, 1933, **16**, 25–57.

20. CARPENTER, C. R. Psychobiological studies of social behavior in Aves. II. The effect of complete and incomplete gonadectomy on secondary sexual activity with histological studies. *J. comp. Psychol.*, 1933, **16**, 59–97.

21. CARR, G. Condillac's treatise on the sensations. Los Angeles: Univ. Southern California, 1930.

22. COGHILL, G. E. Anatomy and the problem of behaviour. Cambridge, Eng.: Univ. Press; New York: Macmillan, 1929.

23. COGHILL, G. E. The neuro-embryologic study of behavior: principles, perspective, and aim. *Science*, 1933, **78**, 131–138.

24. COGHILL, G. E. Integration and motivation of behaviour as problems of growth. *J. genet. Psychol.*, 1936, **48**, 3–19.

25. CORONIOS, J. D. The prenatal development of behavior in the cat. *Genet. Psychol. Monogr.*, 1933, **14**, 283–386.

26. CRAWFORD, M. P. The relation between social dominance and the menstrual cycle in female chimpanzees. (Abstract.) *Psychol. Bull.*, 1940, **37**, 432.

27. GUTHRIE, E. R. Association and the law of effect. *Psychol. Rev.*, 1940, **47**, 127–148.

28. GUTHRIE, E. R., & HORTON, G. P. A study of the cat in the puzzle-box. (Abstract.) *Psychol. Bull.*, 1937, **34**, 774.

29. HOOKER, D. Early fetal activity in mammals. *Yale J. Biol. Med.*, 1936, **8**, 579–602.

30. JASPER, H. H., BRIDGMAN, C. S., & CARMICHAEL, L. An ontogenetic study of cerebral electrical potentials in the guinea pig. *J. exp. Psychol.*, 1937, **21**, 63–71.

31. KLÜVER, H. Behavior mechanisms in monkeys. Chicago: Univ. Chicago Press, 1933.

32. KÖHLER, W. Gestalt psychology. New York: Liveright, 1929.

33. KUO, Z. Y. Ontogeny of embryonic behavior in Aves. V. The reflex concept in the light of embryonic behavior in birds. *Psychol. Rev.*, 1932, **39**, 499–515.

34. KUO, Z. Y., & CARMICHAEL, L. A technique for the motion-picture recording of the development of behavior in the chick embryo. *J. Psychol.*, 1937, **4**, 343–348.

35. KUSSMAUL, A. Untersuchungen über das Seelenleben des neugeborenen Menschen. Leipzig: Winter, 1859.

36. LINCOLN, A. W. The behavioral development of the feeding reaction in the white rat. Master's Thesis, Brown Univ., 1933.

37. MINKOWSKI, M. Zur Entwicklungsgeschichte, Lokalisation und Klinik des Fussohlenreflexes. *Schweiz. Arch. Neurol. Psychiat.*, 1923, **13**, 475–514.

38. MINKOWSKI, M. Neurobiologische Studien am menschlichen Foetus. *Handb. biol. ArbMeth.*, 1928, Abt. V, T. 5B, H. 5, 511–618.

39. MOWRER, O. H. "Maturation" vs. "learning" in the development of vestibular and optokinetic nystagmus. *J. genet. Psychol.*, 1936, **48**, 383–404.

40. Muller, H. R., & Weed, L. H. Notes on the falling reflex in cats. *Amer. J. Physiol.*, 1916, **40**, 373–379.
41. Orr, D. W., & Windle, W. F. The development of behavior in chick embryos: the appearance of somatic movements. *J. comp. Neurol.*, 1934, **60**, 271–285.
42. Parker, G. H. The elementary nervous system. Philadelphia: Lippincott, 1919.
43. Pfaffmann, C. Differential responses of the new-born cat to gustatory stimuli. *J. genet. Psychol.*, 1936, **49**, 61–67.
44. Preyer, W. Die Seele des Kindes. Leipzig: Fernau, 1882. (5th ed., 1900.) The mind of the child. Pt. 1: The senses and the will. Pt. 2: The development of the intellect. (Trans. by H. W. Brown.) New York: Appleton, 1888, 1889.
45. Preyer, W. Specielle Physiologie des Embryo. Untersuchungen über die Lebenserscheinungen vor der Geburt. Leipzig: Grieben, 1885.
46. Raney, E. T., & Carmichael, L. Localizing responses to tactual stimuli in the fetal rat in relation to the psychological problem of space perception. *J. genet. Psychol.*, 1934, **45**, 3–21.
47. Rawdon-Smith, A. F., Carmichael, L., & Wellman, B. Electrical responses from the cochlea of the fetal guinea pig. *J. exp. Psychol.*, 1938, **23**, 531–535.
48. Richter, C. P., & Schmidt, E. C. H. Behavior and anatomical changes produced in rats by pancreatectomy. *Endocrinology*, 1939, **25**, 698–706.
49. Stone, C. P. Motivation: drives and incentives. *In* Moss, F. A., *Comparative Psychology*. New York: Prentice-Hall, 1934. Pp. 73–112.
50. Swenson, E. A. The development of movement of the albino rat before birth. Doctor's Thesis, Univ. Kansas, 1926.
51. Tracy, H. L. The development of motility and behavior reactions in the toadfish (*Opsanus Tau*). *J. comp. Neurol.*, 1926, **40**, 253–360.
52. Warkentin, J. A genetic study of vision in animals. Unpublished Doctor's Thesis, Univ. Rochester, 1938.
53. Warkentin, J., & Carmichael, L. A study of the development of the air-righting reflex in cats and rabbits. *J. genet. Psychol.*, 1939, **55**, 67–80.
54. Warkentin, J., & Smith, K. U. The development of visual acuity in the cat. *J. genet. Psychol.*, 1937, **50**, 371–399.
55. Windle, W. F. Physiology of the fetus: origin and extent of function in prenatal life. Philadelphia: Saunders, 1940.
56. Windle, W. F., & Fish, M. W. The development of the vestibular righting-reflex in the cat. *J. comp. Neurol.*, 1932, **54**, 85–96.
57. Windle, W. F., & Griffin, A. M. Observations on embryonic and fetal movements of the cat. *J. comp. Neurol.*, 1931, **52**, 149–188.
58. Windle, W. F., Minear, W. L., Austin, M. F., & Orr, D. W. The origin and early development of somatic behavior in the albino rat. *Physiol. Zoöl.*, 1935, **8**, 156–185.
59. Windle, W. F., O'Donnell, J. E., & Glasshagle, E. E. The early development of spontaneous and reflex behavior in cat embryos and fetuses. *Physiol. Zoöl.*, 1933, **6**, 521–541.
60. Windle, W. F., Orr, D. W., & Minear, W. L. The origin and development of reflexes in the cat during the third fetal week. *Physiol. Zoöl.*, 1934, **7**, 600–617.

61. Yanase, J. Beiträge zur Physiologie der peristaltischen Bewegungen des embryonalen Darmes. I. Mitteilung. *Pflüg. Arch. ges. Physiol.*, 1907, **117**, 345–383.
62. Yanase, J. Beiträge zur Physiologie der peristaltischen Bewegungen des embryonalen Darmes. II. Mitteilung. Beobachtungen an menschlichen Föten. *Pflüg. Arch. ges. Physiol.*, 1907, **119**, 451–564.
63. Yerkes, R. M. Dominance and sex among chimpanzees. (Abstract.) *Psychol. Bull.*, 1940, **37**, 432.
64. Young, W. C., Dempsey, E. W., & Meyers, H. I. Cyclic reproductive behavior in the female guinea pig. *J. comp. Psychol.*, 1935, **19**, 313–335.

CURRENT TRENDS IN PSYCHOLOGICAL THEORY

BY J. R. KANTOR
Indiana University

Psychologists who critically summarize the literature of their specialty, say, in the manner of numerous *Bulletin* articles and several recent comprehensive treatises—for example, the Murchison "Handbooks" on experimental (**84**), social (**85**), and child (**83**) psychology; Murphy, Murphy, and Newcomb on social psychology (**86**); and Woodworth's *Experimental psychology* (**119**)—must develop two distinctly different impressions. On the one hand, such a reader inevitably becomes optimistic concerning psychology's coming of age. As Woodworth says of his book, such efforts to digest and organize the results achieved in psychology picture for the student "a live progressive enterprise carried on by many enthusiastic workers whose efforts, scattered over a wide field and not coördinated by any higher authority, still reveal an underlying unity of purpose" (**119**, Preface).

This enthusiasm concerns not merely work accomplished or in progress, but extends also to general scientific orientation. Though Woodworth appears to applaud "work for work's sake" and to disparage theories (by assuming a pose of indifference) and schools (by indiscriminate acceptance), it is certain that he is merely objecting to autistic construction which may be not only irrelevant, but obstructive to research. Is it not inescapable that we should differentiate between (a) busywork as unobjectionable self-indulgence, (b) technology as achievement for use and enjoyment, and (c) science, whose purpose it is to attain a fundamental understanding of a selected type of phenomena? Psychological literature reveals that psychologists are extending their horizon to methodological problems, a process which surely augurs well for the better development of psychological science. Students of psychology are becoming increasingly aware of the need for theoretical stocktaking. As psychological data multiply and psychologists need no longer protest that they have a scientific field to work, they may well pause to emulate the constant efforts of physicists, mathematicians, and other well-established scientists to perfect their basic theories.

On the other hand, the paean is not without its discordant notes. Are the conditions of psychology as glowing as they seem to be, or is it possible that the suggested advancement constitutes merely variations within a fixed sphere of cultural patterns? Is it true of psychology that *plus ça change plus c'est la même chose?* Incredible as an affirmative answer may be, there are those who believe that much of the current zealous research eventuates only in supporting various general and special anteriorly accepted doctrines. As to the former, the charge is made that, while many investigations add to our fund of data, the interpretative procedures adopted do not even bring psychology to the point at which it excludes nonnatural materials from its domain. Such skeptics assert that interpretative advances in psychology constitute merely stylistic variations within an historical dualism which maintains itself unceasingly. Similarly, it is held that the various special schools of psychology not only stress merely those types of researches that can be assimilated to their school assumptions, but, moreover, insist upon interpreting experiments or clinical findings in such a way as to establish their own views without regard to the more general claims of psychological science.

Such dissonant notes stimulate an important question. Is psychology still dominated by various hampering traditions? Is it possible that even the present assiduous attention given to theory, as exemplified by an interest in operationism, field theory, and symbolic logic, may not succeed in freeing psychology after all from its entanglement with dualistic doctrine? Admittedly, the present-day reaching out by psychologists toward the abovementioned theoretical techniques need not necessarily result in achieving a desirable goal, since we recall that psychologists have always been intensely sensitive to scientific developments in the general cultural complex of which psychology is a part. Thus, for example, even when psychology was most hopelessly psychic, it adopted the quantitative ideal from astronomy and physics, biological correlates and bases from physiology, and experimentation from the various laboratory-using sciences. It is the purpose, then, of the present article to review some representative literary items in order to ascertain the relative potencies in psychology of cultural tendencies and investigative operations. Accordingly, I consider briefly the theoretical implications for psychology of the following eight (not unrelated) scientific developments: (1) field theory, (2) the operational conception, (3) numeralization, (4)

semantics, (5) symbolic logic, (6) psychomathematical theory, (7) physical analogism, and (8) psychoneurology.

I. Field Theory

Although authentic field theory is basic to science, psychologists have become sensitive to it only after relativity developments in physics forced the field theory into such prominence as to become a general intellectuo-cultural tendency. Despite the tardy appearance of field formulations in psychology, appreciable numbers of psychologists, however, have recently become interested in them. The question is in order, then: Have field formulations helped to further psychology as a science? Naturally, the effectiveness of a scientific theory depends upon how it is interpreted—in other words, how one interbehaves with or uses it. What, then, is a scientific field?

The Nature of Field Theory

Fundamentally, a scientific field consists of a definite frame of reference marking the limits of interactions of phenomena during the occurrence of events. In some cases this reference frame may be regarded as merely the boundaries of an isolated body whose behavior (motion) is being studied so that we can determine what action (gravity, elasticity, friction) other bodies exert upon it. Again, in studying the volume (V) or energy (U) of a fluid we observe changes in these variables as functions of the coördinates P = pressure and T = temperature, so that the volume and energy may be regarded as factors in a mutually interbehaving system.

Generalizing, every scientific description consists of the organization of two or more variables. Velocity in general may be described as a ratio of s and t according to the formula $v = ds/dt$. Again, force may be defined as mass times acceleration. In stating the simple law of the lever the interrelation of four variables is required: $l_1m_1 = l_2m_2$.

Whence are these variables derived? All scientific records and descriptions constitute arrangements and constructions. This is as true of the most elementary symbolization of phenomena as of the most elaborate theoretical descriptions or explanations. The question, therefore, arises: Are the variables derived from operations upon the crude or preanalytic data, or are they imposed upon the observed original events on the basis of tradition? Physicists

claim that they have rid themselves of anthropomorphic forces or causes in favor of the distribution of energies in variously organized systems of masses and motions. In psychology, unfortunately, some of the variables have often been taken as nonnatural—that is, nonspatiotemporal substances, processes, forces, or principles. Doubtless, the significance of field theory in any science consists in its serving as a technique for excluding nonexistent and nonnatural factors.

It is obvious, of course, that psychologists no longer retain manifestly nonnatural variables in their descriptions. But this is not enough. The question is: Do they conceal them behind such screens as the assertion that we must not raise philosophical questions—a technique which Weiss (**110**) has severely criticized? Nor are we rid of such factors by simply declaring that sensation is observable behavior or discrimination of color, for example, and then reducing the discriminand to nondiscriminable abstractions such as light ray "stimuli," with the consequent necessity of having to regain it by magical operations of the discriminator's nervous system. Such an organization of variables hardly satisfies the demands of genuine field theory and is only possible by equating one's free constructions with natural qualities of things, though obviously there is a great hiatus between the discrimination phenomenon studied and the descriptions imposed upon it.

Field Theory Contrasts with Internal Principle Theory

The elimination of mental or psychic variables from psychological descriptions is only one step in field theory development. Another is the exclusion of internal principles. The contrast between internal principles and field theory is excellently illustrated by Kepler's laws of planetary motion, Galileo's law of a freely falling body, and their integration by Newton into the Law of Gravitation. For forces acting upon the planets in the direction of their motion to keep them moving in their orbits Kepler substituted a radius vector connecting the sun and a planet, and instead of an internal principle determining the fall of a body Galileo evolved the dynamic relation of bodies in a field. The stress upon observational distances and times is well exemplified by the $S=\frac{1}{2}gt^2$ formula. Newtonian gravitation generalizes this field principle in the equation $P = f(mm_1/r^2)$, while relativity theory emphasizes specific reference frames and departs from absolute and unrelated space and time.

Examples of internal principles in psychology are exceedingly numerous—for instance, native intelligence, instinct, hereditary individual differences, motivation, and all the powers ascribed to the brain and other structures of the nervous system. Psychology and biology appear to have been dominated by internal principles to a much greater extent than the physical sciences—a fact having its probable basis in the modern derivation of classical physics from celestial dynamics. Gravitation was found to be proportional only to the inertia of bodies and independent of their composition and temperature. Because electrical and magnetic phenomena require such modification of our idea concerning mass and energy as to allow for their transformation, the development of microscopic physics powerfully reinforced the field theory. With the inevitable interest of biology and psychology in the structural organization of the behaving unit the way is smoothed for the maintenance of internal principles.

Types of Field Theory

We have already implied that the field conception is much more widespread than would appear from the lack of references to it. Many psychologists of different persuasions have adopted some form of field theory without using the name. Needless to say, these are not all of equal advantage to psychology. Here are four illustrative types, several of which may be espoused by the same writer.

(*a*) *Organismic Field Theory*. This theory has been derived by the psychologist from the biologist's conception that organismic phenomena may be regarded as activities performed by organisms in interrelation with external conditions called "stimuli." This conception can be traced back to the biologist's study of the interrelated problems concerning units and general behavior. Since the establishment of the cell theory, some biologists have insisted that not intraorganic cells constitute biological units but, rather, the organism as a whole (**95**). Again, with the development of such animal behavior studies as Loeb's (**75**) observations on tropisms and the general investigations of the ecological relations of organisms and environment, a further impetus was given to an holistic or synthetic conception of organisms. The emphasis upon the unity of the organism and upon the behavior of the organism-as-a-whole, with the inevitably correlated environment, is bound to lead to some type of field formula. Type and rate of growth, re-

generation, disease, or maladjustment are describable as the interaction of cellular structure and such surrounding conditions as chemical, mechanical, thermal, or electrical charges.

From these biological field studies a simple transition can be made to the elaborate investigation of locomotory and discriminating actions of animals under definite light and mechanical conditions and interferences, as by Crozier (**24**), Mast (**77**), Hecht (**39**), Hoagland (**25**), and others. Hardly a step beyond lie the innumerable studies of Pavlov, Bekhterew, and their followers on conditioned interactions with stimulus objects.

Unfortunately, biologists do not achieve a field theory in the same degree as physicists. It is interesting that, whereas field theory might be especially favored by biological mechanists, vitalists have also espoused such ideas. Spemann (**99**) points out that Gurwitsch (**37**, **38**) has elaborated the field idea which he borrows from physics, so that the "field has a far-reaching independence of the material embryo," while Weiss (**111**), too, has made his "determination field" independent of its biological substrate. Weiss (**112**), however, may not agree with this interpretation of his theory. Spemann (**99**) himself confesses the use of psychical instead of physical analogies. He writes:

> Again and again terms have been used which point not to physical but to psychical analogies. This was meant to be more than a poetical metaphor. It was meant to express my conviction that the suitable reaction of a germ fragment, endowed with the most diverse potencies, in an embryonic "field," its behavior in a definite "situation," is not a common chemical reaction, but that these processes of development, like all vital processes, are comparable, in the way they are connected, to nothing we know in such a degree as to those vital processes of which we have the most intimate knowledge, viz., the psychical ones. It was to express my opinion that, even laying aside all philosophical conclusions, merely for the interest of exact research, we ought not to miss the chance given to us by our position between the two worlds.

On the whole, whether or not biologists involve themselves with vitalistic processes, it seems clear that even such mechanistic biologists as Loeb (**74**), Child (**23**), or Herrick (**41**) primarily localize their causative or explanatory factors within the organism in the form of such internal principles as metabolic gradients or the unitary character or wholeness of the organism. Probably at best Spemann's indicators and organizers fit into the same scheme of things. In a sense, then, the organismic theory is only an approximation to a field conception and at most a link between an authentic field and internal principle theory.

The generally unsatisfactory status of biological field theories may probably best be accounted for by the intricate and, to a great extent, unobservable character of the phenomena. Especially is this true for general and intercellular embryological phenomena. Psychology is in a better position, since psychological fields are more readily open to inspection. Yet, organismic field theories in psychology follow their biological prototypes. There is lacking, then, a definite basis of interbehavior between an organism and actual stimulus objects. Stimuli are regarded as merely occasions, cues, or clues which direct the organism's activities, but which are not fundamental factors in psychological events. Justification for this view, it must be admitted, is available in such physiological data as those formulated by the all-or-none law of neural or muscular energy discharge.

Generally speaking, internal principles are localized by psychologists in neural functions, as is strikingly the case with Goldstein (**36**), Holt (**43**), and Hull (**44**, **45**). Skinner (**97**, **98**), however, flatly rejects the potency of neural explanations of behavior. In general he minimizes the role of stimuli as exclusively antecedent elicitors of responses and stresses the spontaneous emittance of behavior.

(*b*) *Psychanalytic Field Theory.* Psychanalysts who aspire to a dynamic view stress the effect upon individuals of contacts with various untoward conditions. It is undeniable that dealing with normal and abnormal psychological phenomena in terms of contacts individuals have had with previous persons and conditions, and the cumulative effect of such contacts upon present behavior, constitutes an excellent beginning for a behavior dynamics. Murray and his co-workers (**87**) have indeed worked out an elaborate dynamics of this type for normal personality study. Brown (**15**) has evaluated the extent to which Freud employs field notions as compared with what Brown calls "class theory," which, if not identical with, includes internal principle theory. It may be granted, then, that up to a point psychanalytic studies proceed upon a useful field study plan.

At most, however, psychanalysts treat psychological phenomena as behavior of persons in psychological fields merely upon an elementary descriptive level. In other words, they concern themselves with fields only as far as crude data are concerned. Essentially, psychanalysts, in so far as theyexplain and interpret psychological phenomena, not only stress internal principles, but these internal principles are chiefly psychic. Psychanalytic writers oper-

ate more with psychic determination, unconscious forces, mythical life and death instincts, the ego, id, and superego, than with actual persons in personal, domestic, economic, and general cultural situations.

(*c*) *Gestalt Field Theory*. Probably the chief contribution of the recent gestaltists consists of moving beyond the simple holistic view of the psyche or mind held by their intellectual ancestors (Brentano, Stumpf) to a general structuralization of events. Though the recent gestaltists do not abandon the holistic maxim that the whole is greater than the sum of the parts, they adopt a configurational view that approximates a field theory. Köhler (**59**) especially attempts to derive his gestalt ideas from a general or cosmic dynamics, while both he (**60**) and Koffka (**58**), and many others (**81**), have described learning and perceiving as definite interactions of organisms with specific objects. In planning and executing numerous experiments upon color and form constancy, figure and ground, the effects of complete and incomplete or structural and unstructural situations upon learning, the gestaltists have certainly indicated the possibility of authentic field descriptions.

Yet, on the whole, gestalt field theory misses the goal of objective interrelations of factors. Except on the level of crude or preanalytic data gestaltists do not deal with authentic fields. A serious defect in gestalt theory is the abundant employment of the phenomenal. The term phenomenon is clearly used in continuation of the psychistic notions of experience and consciousness. An excellent example is Koffka's behavioral space, which he differentiates from physical space (**58**). It is impossible to interpret this otherwise than as a purely metaphysical conception.

An interesting side light on gestalt theory is the terminological or symbolic argument. Since it is clear that the relativity of psychological interactions requires a different space formulation from that of classical physics, resort is made to the Riemannian doctrine of the n-dimensional manifold (**17**). Williams (**117**) especially declares that behavior space is not in the Euclidean x, y, z world any more than is the Minkowski Einsteinian x, y, z, t space. He goes on to declare that "behavior space is constituted of our experiences" (**70, 117**). Visual, auditory, memorial, tactual, and imaginal experiences are made the terms of a manifold, and in this way a glorious achievement of the geometricians is made to do dubious psychic duty.

If phenomena in the sense of psychic or experiential descrip-

tions are opposed to authentic field theory, then gestalt field theory is not the genuine article. No amount of terminological usage can alter this fact. Moreover, not only have gestaltists not departed from psychic explanations, but the explanatory principles are internal rather than field. Impressive it is how large a place gestaltists allot to values, fitness, and requiredness (**61**). The gestaltists' traffic with internal principles is emphasized by their conformity to the conventions of the time in translating their psychic internal principles into neural traces. At most, then, like the psychanalysts the gestaltists deal with fields on a crude-data level, but not in their fundamental scientific construction.

In fairness to some of those who at the same time incline toward, and deviate from, the gestalt school—for example, Lewin (**69**) and after him Brown (**17**)—it must be said that there is an attempt to deal with psychological fields in an essential and systematic way. Unfortunately, the writers mentioned set up their fields in imitation of physical and mathematical systems, so that they become merely formal and analogical symbolic structures with little or no actual descriptive or interpretative value for psychological phenomena (**52**).

(*d*) *Interbehavioral Field Theory.* Assuming that an authentic field theory requires the extrusion of psychic and internal principles, can such a field theory be attained? Nothing stands in the way. One essential requirement, however, looms large, namely: such a construction must be definitely derived from the actual interbehavior of organisms with objects and events in specific situations (**53**). Fields, then, are essentially loci of interbehavior of response and stimulus functions on the basis of a continuous interrelationship of successive contact conditions between organisms and their stimulus objects. Only those who believe that a scientific theory must bear no resemblance to the original data will have difficulty with this suggestion. Such theorists believe that physics must be reduced to geometry, biology to chemistry, the world of color and sound to vibrations, and, in general, everything must be reduced to numbers. The inevitable result is to introduce epiphenomenal psychic qualities in order to maintain a link with nature. The phenomena are virtually unreal and must, in turn, then be anchored to neural functions to prevent science from disappearing into evanescent non-space. How much the argument that science is constructive can help matters is hardly an open question.

In the interbehavioral field-formulation not only are all psychic and internal principles excluded, but also the neural tissues and functions are allowed no surrogatory explanatory function in psychological description.

II. Operationism

New ideas in science frequently suffer a serial fate, namely: first they are ignored, then pronounced of no value, but in the event they maintain themselves it is forthwith declared that they are so obvious that everybody has always held them. Perhaps because the operational principle stems from physics it has received a definite hearing from psychologists, though it seems that it is being assimilated only by each writer interpreting it according to his cultural kind. By examining some typical attitudes toward operationism we can easily detect the influence of cultural patterning upon the reactions of psychologists.

Operationism as Simply Scientific Work

Certainly there is a firm foundation for the attitude that operationism is nothing more than the acknowledged practice of scientists during observation and experimentation (**13**, **54**, **79**). Yet this attitude overlooks the actual significance of the operational principle. Every proposition or statement in science, every technique or principle, has significance only in its particular context or life condition. Accordingly, since our current operational discussions were initiated by Bridgman (**12**), it would be well to consider upon what ground he stood. There is no doubt that for Bridgman the fundamental significance of operationism is that it serves as a check upon the arbitrary construction of mathematical equations in physics (**54**). In addition, it is proposed that to erect scientific constructions effectively one should make certain of a continuity between the constructions and the crude data. In this way one can clearly differentiate between purely conventional constructions and descriptions based upon actual interbehavior with genuine events.

On this basis the operational principle constitutes a distinctive and important formulation deserving careful consideration. The psychologist may well regard his operational problems as more serious than those of the physicist. By contrast with the latter, who merely runs the risk of excessive extrapolation by pyramiding equations, the psychologist may start without a factual basis alto-

gether. A pertinent and important question is whether such constructions as consciousness, sensation, drives, etc. have in any sense been derived from interbehavior with phenomena rather than from traditional theory.

Operationism as a Defining Technique

An important assimilative reaction toward operationism is to regard it as a scheme of definition, so that traditionally objectionable concepts and terms can be made palatable. For example, Boring (**11**) apparently assumes that operationism is merely a way of verbally defining things. He says:

> We now know that operational definitions can always transform a psychological description, expressed in terms of consciousness, into a description in behavioral or physiological terms.

Similarly, Bills (**10**) asserts:

> Parallelism would automatically reduce to a double-aspect formula, because where two sets of defining operations coincide perfectly they become identical operationally.

Again, Stevens (**102**) believes that "mentalistic" concepts like percept, image, and idea can be operationally defined.

It may be strongly urged that if we are to retain any conception or construction as a descriptive factor in a scientific system, it must be derived from an investigative contact with actually occurring events. It is obvious (**50**) that nobody has ever operated upon psychic or mentalistic states or processes whether named sensations, images, ideas, or perceptions. No psychologist has ever investigated anything but the interbehavior of an organism with stimulus objects and conditions presented to that organism. It can only be a surrendering concession to tradition to justify consciousness or sensations by declaring that in the end, like all scientific constructions, they are useful concepts or, after all, physiological (neural) events.

Operationism as the Invocation of Manipulative Procedures

A more misleading, because more subtle, attitude is to assume that if manual operations are performed any accepted theory is justified. By virtue, then, of manipulations or experiments innumerable psychic variables are admitted into psychology. The underlying thought-technique here is to assume that if one can observe or measure some feature of a response one may assume a

mental process. The classic psychological examples of this technique begin, of course, with Fechner's interpretation of the theological construction of the universe from his psychophysical experiments. Not the least vulnerable point in this view is that it overlooks that, in the same sense, observing an event or formulating a proposition are also operations. What is desired, of course, is a scientifically significant operation.

The writer (**50**) has frequently pointed out that there may be a great hiatus between experiments performed and the constructions built upon those experiments, and even between what the experimenter regards himself as doing. But unfortunately the difficulty lies deeper. Not only does the fact that one misinterprets one's operations perpetuate unscientific traditions, but also the grip of these traditions leads to a misinterpretation of what one is interbehaving with. Thus, in measuring nerve currents are we measuring correlates of mental processes or phases of reaction (discrimination) to certain objects or events, say, sounds? Probably a criterion for genuine operational data and procedures is whether or not one requires a double-aspect or identity dodge or organic bases or correlates to get rid (verbally) of dualism.

All too frequently psychologists preserve the dualistic tradition by a unique translation of words or terms. Bills (**10**) says he cannot share Kantor's opinion that the act of discriminating cannot be a real experimental operation. Kantor has never held such an opinion. Rather, what he has insisted upon is that discrimination must always be regarded as a response of the organism to colored or sounding objects and not a complex of "experiences" and/or neural or general physiological action. The fundamental question here is whether such terms as sensing, perceiving, or experiencing represent or involve color, sound, or other sensation qualities created after or beside neural action, or whether colors, sounds, etc. are qualities or properties of things reacted to by organisms which have evolved specified receptional, neural, and other biological traits.

Operationism as a Postulational Device

The recent zealous discussion of operationism has been decidedly beneficial in bringing to the foreground the relations between cultural patterns and psychological theory. Certainly, the sifting of pros and contras has illuminated the ideological background of psychological theory. Has it not revealed that despite all the accumulated data and the forging and sharpening of experimental

tools and methods, psychology is still treated as not a natural science? To speak plainly, as long as psychology is metaphysical in any form—as it always will be while it is psychophysical or psychophysiological[1]—it is still in the grip of historical tradition.

Yet one may still insist that operationism may be an effective agency in promoting a scientific psychology freed from metaphysical entanglements. But what is metaphysical? Stevens (**102**) asserts that my various attacks on dualism imply a realism which is objectionable because it is metaphysical. An interesting question is whether any view, as long as it is decidedly not dualism, is not preferable to the latter, even though one calls it a metaphysics. On the other hand, it is plain that the formula, "Nature is the same as our knowledge of nature," is equivalent to Berkeley's spiritual "esse est percipi," whether regarded as a general ideological proposition or as a specialized scientific conclusion such as Stevens reaches in his work on Hearing (**103**). In that work he writes:

> The centers seem to code their messages in a new language and we are still seeking the clue to its translation (p. 307).

In addition to the implied dualism the term message, despite current physiological usage, suggests a palpable anthropomorphism.

Is it an objectionable realism to insist that one of the basic postulates of psychology as well as of all other sciences is that the scientist begins with crude or preanalytic data in which he somehow becomes interested and which he manipulates with a later construction of records, descriptions, analyses, and interpretations in the form of symbols, graphs, or words? In psychology, we repeat, we can start with our interbehavior with crude data—an observed subject's discrimination of sound, color, shape, or objects, his reaction time to a stimulus object, or his attitudes toward war, etc.—and build up a descriptive and explanatory structure of abstractions derived from such concrete observations.

Quite properly Stevens raises the question whether any operations can be formulated to test a metaphysical proposition. The answer is, of course, emphatically "No." The question in the present context arises only because antimentalism is called realism and hence metaphysics. The kind of naturalism I espouse, even

[1] As a pedagogical device the writer has long been insisting that, whether one adopts the frankly dualistic formula ψ=psychological phenomena$=M+B$, or the behavioristic $\psi=B-M$, or the aspectual compromise $\psi=(MB)$, we are still harboring nonspatiotemporal, *i.e.* nonnaturalistic, factors in our thinking.

if it is called metaphysics, is not only operationally testable, but is proved in the sense that all science proceeds in that way. Stevens and I agree that scientists should dispense completely with all traditional metaphysics, but we differ on the proposition whether sensations and other mental states neurally mediated are metaphysical (Berkeleian spiritualism), even though psychologists and physicists retain such constructions.

I repeat: If the naturalistic view that all science at bottom constitutes the observation of, and experimentation upon, events, whether the interbehavior of two bodies (sun and earth, for example) or an organism and a sound, must be called realism, then realism or metaphysics is simply another name for science. To me, the fundamental question is whether we build our science upon actually existing events in terms descriptive of those events or whether factors derived from nothing more than historical tradition ["sensations produced through the ear" (**103**, p. 455)] slip in to mar our scientific formulations.

III. Numeralization

The progression of numerological ideas excellently delineates the polarity between adherence to cultural patterns and fidelity to facts. Since the peak of numeralization in psychology has passed it has become a matter of historical interest only to point out the indulgence of psychologists in such quantifying excesses as the substitution of numbers and numerically symbolized artifacts for natural phenomena—as, for example, the learning curve—or, what is just as bad, the neglect of essential events because they are not numeralizable. Instead, one may hail the growing literature denouncing the numeralizing tendency of regarding scores, frequencies, or correlation coefficients as more important or more basic than the facts they are used to represent.

The Power of Numbers

Whether or not it is proper to trace back the perennial straining after numbers to Pythagoras, certainly that ancient thinker showed the way to making numbers the underlying reality of all phenomena. Since Pythagoras grasped the relation between simple ratios and musical intervals there has been a continuous attempt to reduce qualities to quantities and to establish that all is number.

Psychological literature is becoming replete with protests against sacrificing data and principles to number tradition. Carr

(**22**), Johnson (**47**), Melton (**80**), Bartlett (**6**, **7**), and Myers (**88**) have pointed out such methodological errors as neglecting essential features of events and their conditions in order to achieve formulae. Such errors have been especially patent in the fields of tests and learning, though other investigative domains are not exempt. No one, of course, will confuse a criticism of the misuse of numbers with a minimization of the value of mathematical or statistical procedures. Every psychologist can accept Kelvin's dicta:

> When you can measure what you are talking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meager and unsatisfactory kind. It may be the beginning of knowledge but you have scarcely, in your thoughts, advanced to the stage of science, whatever the matter may be.

He need only remember that Kelvin was an engineering physicist and that measurement consists of definite operations with a particular kind of subject matter. On the other hand, we cannot overlook the statement of the chemist Lewis (**73**):

> I have no patience with attempts to identify science with measurement, which is but one of its tools, or with any definition of the scientist which would exclude a Darwin, a Pasteur, or a Kekulé (p. 6).

To this list may be added the names of Harvey, Linnaeus, Schwann, Schleiden, Virchow, and many others.

What we should insist upon is maintaining an interest in, and keeping in touch with, phenomena through the best observational techniques we can develop, among which we number numeralization and statistical manipulation, without making fetishes of curves, numbers, and numerical techniques.

Psychometrics

Curiously enough, the adoption of the so-called operational definition of intelligence that intelligence is what intelligence tests measure has helped to turn psychology away from the concrete performances of testees as intelligence phenomena to test scores as a substitute. An especially vexing consequence is the failure to observe that numerical data can be of no greater importance than the performances from which they are derived. Thus, it must always have appeared striking how great are the results concerning the complex phenomena of human intelligence that can be derived from the administration of simple task-tests.

Among the encouraging psychometric suggestions that what numbers represent is important rather than the numbers themselves are those found in a number of papers by Thurstone (**106**, **107**). This writer recently has been insisting upon the important distinction between factor analysis and other statistical methods that do or do not make psychological sense. Similar suggestions concerning the care needed to use mathematical techniques in such a way as to prove fruitful in psychological studies have been made by Tryon (**108**, **109**) and Anastasi (**4**). Especially have these suggestions been directed toward the elimination of faculties and other intangible psychological principles from the field of intelligence and individual psychological differences.

Representative of the general criticism of mathematical procedures in the test field are the papers of Kelly (**56**) and Peatman (**91**). These writers, among many others, have proposed critical analyses of mathematical and especially statistical concepts in order to make certain that the psychological materials for which they stand will be given adequate consideration. Such studies articulate with others devoted to the examination of IQ problems and the nature and function of tests. Particularly pertinent are the papers of Kent (**57**) and Wellman (**114**, **115**). The former, especially, raises important questions concerning intelligence as a mathematical variable and the mental test as an instrument of precision. It is now frequently suggested that to quantify badly conceived variables inevitably leads to results which are increasingly precise in their misguidance.

IV. Semantics

In Signis Vincimus

Psychologists, like other scientists, have been influenced by the intense resurgence of symbology in our time. Symbolic attitudes vary from the specific attraction to, and repulsion from, certain words to the general apotheosis of signs and symbols. In general, a pervasive interest in language and the importance of linguistic factors in science characterizes the current cultural scene.

Despite the tinge of medieval nominalism reflected in the current interest in signs, it may be an agreeable speculation that, at bottom, linguism in psychology aims at a commendable objectivity. If we trace modern science back to its Renaissance beginnings, we may easily interpret the present interest in mathematical and other symbols as a continuation of the preference of sixteenth-

and seventeenth-century thinkers for mathematical modes of description as against qualitative (subjective) essences. But at the same time it is inescapable that neither in the Renaissance period nor today have scientists by this means succeeded in abolishing subjectivity (psychic qualities). On the contrary, linguistic techniques always serve to perpetuate mentalism. The physicalists (**20, 21**) have clearly never rid themselves of this incubus and only propose the employment of the *language* of physics, while Stevens (**102**) refers to the ineptness of physicalist language as "suggesting the primacy of a materialistic physics."

Semantics may be defined as the proper relating of referents and references (**9, 62, 90**). The novelty in this discipline is to bring into prominence a procedure inevitably implied in all scientific work. Semantics reduces to proper verbal or symbolic definition. Whether or not we regard semantics as a new discipline we cannot overlook its utility. When taken as a specific analytic procedure of terminological criticism instead of a general philosophy, it has the merit of cautioning us concerning the use of our terms. Semantics, then, constitutes a simple technique for precision in description. At most, however, semantics is useful in publicizing scientific results. Fundamentally, precision and validity of scientific description depend upon proper contacts with the events of a scientific field. To avoid the influence of conventional doctrine the scientific worker must not merely be on his guard against compromising terms and sentences, but also make sure that he is properly observing and analyzing his data. Terms and symbols have no values of their own, but only those the scientist accords them.

Interbehavior with events is the main feature of scientific investigation and basic to all description. Semantic analysis can only be helpful when the scientific worker desires to make his descriptions conform to his tradition-free operations. No scientist is primarily a student of terms. Moreover, if a psychologist, for example, maintains in his thinking such alleged phenomena as subjective states, no terminology such as neural currents or wave frequencies will make him a natural scientist. On the other hand, a psychologist who really interbehaves with a subject's contacts (differential responses) with colored stimulus objects is not at a disadvantage even if he uses such traditionally badly interpreted terms as sensation.

Even on the most elementary level—that is, in face-to-face conversation—language is a human-behavior event distinct from

the things referred to. Always there are various styles of referring to referents. Ordinarily we do not confuse such linguistic phenomena with the situations in which they operate. No more may we do so when the linguistic phenomenon is very remote from conversational speech, as in the case of symbolic record-making and when the recorded events are removed from everyday occurrences. Because a galvanometer reading becomes the index to the amount and direction of current flow we sometimes identify the readings or the descriptive equations with the original phenomena. In this way we confuse science as an operational observational procedure with symbols or other linguistic records of our observations.

V. Symbolic Logic

As the night the day, so must symbolic logic follow in the train of semantics. In this era of signs it is not surprising, then, that biologists and psychologists (**45**, **82**, **118**) make excursions into the realm of symbolic logic. For the most part, symbolic logic is not differentiated from semantics, since the plea is made that symbolic logic helps in developing and manipulating precise terminology. Nor are these terminological claims to be ignored. Surely, it is as important for a scientist to be as precise and accurate in his records and constructions as in his observing, measuring, and weighing. And naturally, whole libraries have been written to enforce appropriate admonitions. Unfortunately, this universally accepted ideal has prevented neither confusion nor disagreement. Why? Certainly, it is not that we lack an absolute method of discovering what terms one should use in the absence of a Platonic realm of immutable and correct terms. We propose, rather, that whatever difficulties of precise description exist in psychology are not owing to the absence of proper words or symbols, but, instead, to faulty interbehavior with phenomena. And such faulty interbehavior is much more a function of cultural influences than a lack of accurate techniques and refined procedures. A most interesting illustration we find in Miller's (**82**) dissatisfaction with Hull's (**45**) logical system because it is behavioristic and not mentalistic.

As to the assimilation of symbolic logic by psychology there are the usual difficulties of such procedures. Symbolic logic is an old discipline, even if traced back only to Leibnitz and not Aristotle (**72**). It is not unusual, then, to find psychologists adopting doctrines which symbolic logicians have long since weighed and found

wanting. Furthermore, developments are so rapid in recent symbolic logical studies that one is perhaps required to specify which system. Quine (**93**) has pointed out:

> The less a science has advanced the more its terminology tends to rest upon an uncritical assumption of mutual understanding. With increase of rigor this basis is replaced piecemeal by the introduction of definitions.

The same thing is true when a psychologist adopts symbolic logic.

What is symbolic logic? Miller, who proposes not only to purify Hull but also to correct him, mentions the work of Veblen and Pieri in geometry. Is there a similarity in problem between the job of a geometer and a psychologist? As our reference to Miller's dissatisfaction with Hull's attempt at logic would indicate, there must be a difference. For one thing, is psychology a deductive science as so many (perhaps wrongly) believe geometry to be?

It is instructive to turn briefly to Pieri (**3**), who perhaps first employed the term "hypothetico-deductive system." What he perhaps properly aspired to accomplish was to squeeze out of geometry the intuitive (in the mathematician's sense) factors which so richly permeated Euclidean geometry. This he did by attempting to build up a metric geometry on the basis of only two undefined terms, namely: point and motion or rigid displacement. The fundamental principle of a hypothetico-deductive geometry is to get away from the space of experience and to deal exclusively with relations between arbitrary entities set up only by postulational or constructional procedures. Can psychology be such a deductive science? It is doubtful whether even a significant operational, as against a purely rational (term-relations), geometry can be built up in this manner.

So far as concerns psychology, then, we may admit that by following the hypothetico-deductive procedure it is possible to test whether a psychologist has achieved a deductive system. But are we merely interested in knowing whether he has properly set up (a) primitive notions, (b) definitions derived from those primitive notions and successively developed, (c) primitive statements, and (d) theorems (**82**)? What about the phenomena that all psychological systems are designed to elucidate?

If psychology is not merely an abstract formal system on the basis that "logic may be the science of possible systems viewed merely as types of structure" (**33**, p. 11), but an investigative

science, then undoubtedly psychology requires another kind of logic. Is it true that symbolic logic is at all competent to deal with a concrete investigative science as it perhaps can in the case of a science of abstracted relations like geometry?[2]

Implied in this assimilation of symbolic logic is the acceptance of a modern variant of philosophical rationalism. At least it may be suggested that deduction implies an extreme formalism which is never salutary except when dealing with abstract entities. When it is presumed that such entities are derived from formalizing concrete phenomena a gap is usually produced between the abstractions which are symbolically indexed and the original events, so that the ensuing deductions are fatal to the phenomena.

It is quite apparent that the assimilation of symbolic logic is proposed by Miller not so much as an instrumental aid for psychological work as a new tool for establishing his own type of psychology. Certainly, Miller, in criticizing Hull, is invoking symbolic logic to make room for consciousness, awareness, and experience rather than to put Hull in line with symbolic logic. Not a single sign is evident that the criticism of the defects in Hull's system requires the use of Miller's symbols or ideology. If, for example, the investigation of psychological phenomena reveals that they are mentalistic, the investigation and not symbolic logic reveals it. In this example psychology, after all, is not so much concerned with deductive systems, as Hull also believes, as with the nature of certain kinds of phenomena both as observed in the field and manipulated in the laboratory.

There remains still the question of descriptive terms. Granting that psychologists have investigated the nature of psychological phenomena and are satisfied concerning the events, what sort of descriptions should they make? There is only one rule, namely: that the descriptions should fit the events described. For example, qualitative or qualitatively known data must be qualitatively described, while quantitative descriptions must be given if the data allow. Beyond this the kind of presentation is a matter of taste. As we see, there are two aspects of descriptions: one points in the direction of the data and the other to the style of referring to them.

[2] Miller has himself distinguished between the availability of sensation and learning for theoretical formulation on the one side and personality on the other. Now if he goes further to consider whether the term sensation should not be a concrete interbehavioral phenomenon, perhaps his faith in symbolic logic might be shaken.

It is a legitimate argument that one must order events to number or space constructions.

Here, circumstances may or may not compel agreement. The problem is, however, a political or social rather than a scientific one. How can Miller, who believes that the validity of a symbological system of psychology depends upon the inclusion of mentalistic assumptions, ever agree with a nonmentalist? Would not the use of dots, horseshoes, waves, and brackets conceal the differences in view concerning the nature of psychological events?

In point in this connection is Johnson's (**48**) recent suggestion that, if it be granted that cause means an interrelation of factors, one may employ either efficient, mnemic, or telic causes, or any combination of them, even though the latter two kinds have not been detected and formulated. Johnson significantly remarks that, if psychologists can truly assert that if "*P* then *Q*" or "if not *Q* then not *P*," they may as well assert "*P* causes *Q*." The significant thing, then, is to describe an observed field of related factors which one can call what one likes. We may add that, if all idea of cause in the sense of an anthropomorphic doing and not doing is eliminated, one may choose not to use the term "cause" in description and experimentation at all.

It is not an unjustifiable faith that psychologists may profit from an occupation with symbolic logic. I, however, am strongly urging that we note the differences between (a) symbolizing things, (b) building deductive systems on the basis of these symbols, and (c) investigating and describing psychological phenomena. This point can be illustrated by Miller's adoption of Murray's (**87**) term "actone." Miller says operational definition must be in terms of actones which are pure action patterns rather than action which implies also the effect of an act. In this way similar actones with different effects (putting food in the mouth and putting poison in the mouth) and different actones (pulling a trigger and taking poison) with the same effect can be distinguished. He adds: "Essentially operational definition must be in terms of actones rather than actions."

Now by psychological analysis and *not* symbolic logic one can easily discover the fallacy in this proposition. It is only when one arbitrarily isolates what an organism does, "putting x in the mouth" (propositional function), from the behavior segment or operational field that one can separate an act from its effect (killing oneself). Operationally there is no similarity between (orally)

interbehaving with food and with poison. Two very different behavior segments are here involved. Also, psychological analysis distinguishes between an effect (killing) in a conventionally social sense and a behavior segment of destroying oneself, which as an interbehavior is similar to looking at something. Miller certainly has not distinguished between reaction systems, behavior segments, and consequences, which need not be psychological factors at all.

Students of logic are aware that one must distinguish between logic as some sort of mystic principle, which has the power of producing correct thinking or investigating behavior, and logic as a technique of system building. In the latter case there is a question of what kind of logic to employ, the answer to which depends upon the problem at hand. There are many other kinds of logic besides the several kinds of symbolic logic. Certainly, we must not fall victim to the venerable convention that the deductive procedure of Euclid or its modern emendations are the paragons of reasoning. Logic must be regarded as a definite human enterprise, and it is possible that logic depends upon psychology as much as psychology depends upon logic (**55**).

VI. Psychomathematical Theory

When Helmholtz declared that "counting and measuring are the bases of the most fruitful, most certain, and most exact of all known scientific methods" (**40**), he indicated one of the most solidly established of all scientific traditions. This dictum doubtless has become so solidly entrenched because the processes it refers to in no way conflict with, or stand in opposition to, the accepted methods of observation and experimentation. Whether or not one admits the traditional character of such fundamental scientific processes, it is certainly plain that mathematical propositions and procedures can be attenuated and abstracted from the operational matrix in which they are so indispensable and built up into abstract theory and even dogma.

In a definite sense our scientific age is one of mathematical models. Scientists are perennially engaged in building up systems of equations to represent phenomena. That this is a decidedly informing and useful procedure cannot be gainsaid, but only when the models are regarded as representative and are not substituted for the original events. This substitution is made possible by correctly assuming that facts are inchoate and insignificant with-

out theory and then substituting the organizing theory for the events to be organized. Naturally, no type of theory lends itself so well to this process as mathematical theory.

Among psychologists, Lewin (69) and Brown (16) have been most articulate in proposing that psychologists employ mathematical theory, and for two reasons: first, because of all types of theory universal assent is most likely to be attained for the mathematical variety, and, second, because mathematical theorizing may be regarded as the prototype of all theorizing in the sense of constructing systems for phenomena. Furthermore, as a justification for apotheosizing constructions above facts, Brown asserts that mathematics is changing from the science of measurement—presumably an operational technique for dealing with phenomena—to a "science whose chief purpose is the construction of logically related systems of concepts." For this reason he asserts that geometry is more important for the building of theories than arithmetic and algebra.

To be sure, mathematical components of scientific theory are not only useful but necessary. To agree to this, however, implies no substitution of facts by analogical organization of concepts. Against such a procedure may be urged the incontestable view that events and contacts with events come first. Events we must observe before any symbolic or mathematical models of them can be made. Observations must be recorded before they can be mathematized, though, of course, we may so set up our laboratory procedure that the observations are obtained only in the form of quantitative measures. The important question, however, is whether we are dealing with abstracted numbers or relations with or without integration with phenomena. Of course, we can assume functions of all sorts and then treat relations between them. An example of this is Rashevsky's (94) proposition:

> Since we are building a purely theoretical science we do not consider any of the above assumptions as having necessarily a counterpart in reality.

That this can be done in no way justifies the transformation of phenomena into mathematical symbols or equations. On the contrary, the procedure merely demonstrates the great range of interbehavior with phenomena. For instance, we need not operate with biological or psychological events, but with abstract relations derived from them. The mathematical models of Herbart and all his host of emulators may be approved of, but the question still re-

mains concerning their connection with psychology. Do we wish to occupy ourselves with models or with psychological phenomena? The choice of occupation is ours, though we have no absolute reference frames with which to determine what is the better kind of occupation. Those who are inclined toward observation and experimentation will be able to discern whether the arbitrary rationalistic procedure leads to a falsification of scientific phenomena. For example, such workers can decide whether by rationalistic procedure we can embrace or reject nonexistent or psychic forces and a dynamics of imaginary factors.

Of the various theoretical trends which we are noting it is not unlikely that psychomathematical theory will meet with the strongest resistance. Garrett (**35**) suggests that he and others find serious objections in topological theory. In reply, Lewin (**71**) practically admits that his vector is equivalent to the older concepts of excitation tendency, drive, libido, and psychological force. Also, Lewin (**69**) admits that his topological theory is intended to be nothing more than a language. For example, he asserts that topological description is meant as mathematical representation for social locomotions such as being initiated into, and expelled from, a group. Unless a style of description or reference is a theory, no one need object to topological psychology. Here it is of interest to note that Gurwitsch (**37, 38**), an early adopter of physical fields and vector theory, frankly intended them as fictions. Furthermore, he acknowledges the influence of the vitalist Driesch, who early formulated the doctrine that the whole is more than the sum of its parts.

VII. Physical Analogism

Closely integrated with the tendency to employ mathematical models in psychology is that of describing psychological phenomena by means of mechanical analogies. An interesting example is Lewin's (**70**) vectoral psychology, which may be characterized as an identification of psychology and geometrico-physical dynamics.

Probably no one objects to the reasons offered to justify this procedure, namely: that all sciences require theoretical formulation, whether this implies the use of a particular kind of terminology or organization of data. At the same time, no one can overlook the questions whether a particular theory (a) constitutes a simple interpretative change, (b) leads to a progressive understanding of events, or (c) merely serves to implement old and inadequate ideas.

Kantor (**52**) has pointed out the inutility of sheer analogism in psychology. With the further development of an analogic psychodynamics as represented by Lewin's monograph on psychological forces (**70**) it is obvious that analogism is merely a support for the traditional mentalism—which hardly furthers our investigation of psychological phenomena. As Lewin himself points out, his vectoral or topological system in no wise is intended to substitute for psychology. He says:

> I cannot imagine how one can stress the necessity and the right of psychological concepts as against any other kind of concepts in psychology more than I have tried (**70**).

As a matter of fact, Lewin (**68**) indicates quite clearly that psychological concepts are concepts of the psychic. Here a significant item is the remark of Adams and Zener (**68**, translators' preface) that a more appropriate translation for Lewin's term *Seele* would be "soul" and not "mind," which they used in deference to American psychological opinion. Though these translators distinguish between a psychological and a theological soul, their assertion that the psychological soul is to be taken in McDougall's sense certainly mitigates the distinction.

A somewhat more methodological form of physical analogism is Brown's stress of hypothetico-deductive systems. This writer similarly fortifies himself with commendable assertions concerning the need of theory in psychology. Brown believes that the espousal of mathematical forms of description by physical scientists requires that psychologists do likewise. Granted that in mechanics it is feasible to reduce objects or bodies to masses, masses to particles, and particles to mathematical points, so that they can be symbolized in equations, does it follow that this sort of analogism is equally useful in psychology? Again, though nature is continuous and homogeneous, may we in theory eliminate the differences between psychological and other kinds of phenomena? Can we in psychology match the mechanical adoption of the mathematical principle that motion is an infinite succession of states of rest and hence reduce dynamics to a branch of statics (d'Alembert's principle)? For certain mathematical purposes such as adding the number of reactions performed we may reduce even such different responses as fear and discrimination to identical members of a homogeneous class, but what kind of theory can we build upon this basis?

In what sense is science hypothetico-deductive, and how can

such a method be adopted in psychology? We have already indicated that this method, taken in Pieri's sense, is inapplicable to so definitely a content-science as psychology. Brown, however, commits himself to a form of rationalism and emphasizes the priority of laws as compared with investigations or measurements (**14**).

In justification he asserts that the hypothetico-deductive system is the method of Galileo as a typical scientist. A study of Galileo's work, however, does not confirm this view. True enough, there are certain survivals of medieval rationalism in Galileo, but so far as his investigative procedure was concerned he did not operate merely to demonstrate laws which he simply and arbitrarily accepted. In his study of freely falling bodies he successively set up two hypotheses, the first that the acceleration was a function of distance and the second that it was a function of time (**76**). In no sense were these pure deductions unrelated to observation, since, as history relates, the latter hypothesis became operationally—that is, experimentally—established.

It turns out, then, that the hypothetico-deductive system is merely Pieri's term applied to the universally acknowledged procedure of scientists. It is only by attributing Baconian method to others that Brown can argüe for the uniqueness and merit of hypothetico-deductive method. Incidentally, however, he does minimize the interbehavior with phenomena which leads to hypotheses or assumptions. This he does by stressing deduction, which he forgets is always interrelated with induction in scientific work. Also, he does not emphasize the fact that laws or hypotheses become modified during the course of investigation. It is only after such modification of hypotheses that they become applicable to other than the original data and form a basis for measurement and prediction.

Fundamentally, then, scientific laws are derived from interbehavior with phenomena. This is as true of the hypotheses or laws of the Pieri hypothetico-deductive type as of those of the natural-event, experimental form. In no sense, however, may we confuse the free postulates set up by mathematicians through their operations with abstract relations with those derived from the more concrete interbehavior with natural objects and events. Though mathematical objects are also derived from natural sources by means of attenuating extrapolation, they are, nevertheless, different on the premise that we must be specific and exact in our descriptions and analyses of operations.

As we have suggested, current methodological discussion in psychology in a manner of its own revives the venerable opposition between the rationalist and the empiricist. As against such a view as Brown's those who insist upon the predominance of empiricism in science have a point which cannot be gainsaid. But such empiricism is really not opposed to a hypothetico-deductive interpretation of science, if by that term is meant an emphasis of hypotheses and postulates as essential guiding processes. Immediately, however, from this standpoint the terms empiricism and rationalism, induction or deduction, should be put on a new basis. All of them must be purged of their mentalistic and dualistic connotations and be taken in a naturalistic or interbehavioral operational sense. Induction and deduction are not, then, as traditionally considered, concerned with either sensing or reasoning. Sensing and reasoning are both absolutely interrelated forms of interbehavior with phenomena. Inductive behavior consists of more intimate inferences concerning observations, while deduction merely constitutes more remote inferences of the same general sort. For example, from observations of a number of subjects' performances we infer the range of their contacts with stimuli, while the achieved range may be employed as a basis for inferring, predicting, or deducing the performance of future subjects.

Similarly, rationalism in science stresses the scientist's activity with results of investigation, whereas empiricism emphasizes the influence of stimulus objects or subject matter in scientific interbehavior. Both of these, naturally, turn out to be merely mutual and reciprocal phases abstracted from single investigative events. For the most part, the overemphasis of rational and deductive processes in science constitutes an overemphasis of results obtained as against the operational processes (observational and experiment) employed to obtain them. But this type of overemphasis is not so serious as substituting unique deductive processes for descriptions of results.

VIII. Psychoneurology

Nowhere more than in the psychologist's dealing with the nervous system is it demonstrated how cultural conventions operate to keep psychology from rigid contacts with data. Thus, we read in Jastrow's (46) volume devoted to *The story of human error* the statement by the eminent neurologist, Herrick (Chap. 8), that:

> neurophysiology occupies a unique place in the circle of the sciences as the point of convergence of the physical and the psychical.

Further, Herrick goes on to say that this branch of science:

> . . . raises the comprehensive problem of man's place in Nature; whether the rich subjective psychic life of man can be brought within the scheme of interpretation applicable to the other sciences, and whether our subjective experience, as the climax of evolution, can be articulated with vital organs and vital processes on a naturalistic plane, and if so on what assumptions.

This statement, which fairly represents conventional psychological doctrine, indicates that despite the writer's undivided devotion to natural science he perpetuates the dualism so antagonistic to his own scientific ideals. The fundamental point here is that as long as the neural structures and functions are made the center, basis, or seat of subjective, psychic life we shall never get away from transpatial forces or functions.

Curiously enough, we have here a neurologist rather than a psychologist insisting upon the genuineness and importance of psychological phenomena as factors in nature. Thus, Herrick insists that mental phenomena control the course of human events. The key to the error here lies in the confusion of crude data—namely, the place of psychological activity in the world of events—with scientifically described data. In other words, Herrick starts with perfectly obvious phenomena and then reduces them to psychic products or correlates of neural functioning. Thus, he reaches the proposition: "The human brain is the organ of civilization."

This error is clear in the author's criticism of behaviorists who, according to him, "refuse to accredit to their own mental experiences any significant role in shaping the course of conduct." Incidentally, the behaviorists are so blind to real phenomena, according to Herrick, because they are dominated by the traditional view of mind as a ghost. The question, then, is whether in making mental phenomena the products of neural functioning he is not himself a victim of this continuance of the ghostly tradition. Herrick believes he can avoid this difficulty by insisting that psychological and biological phenomena are mechanistic. Obviously this is merely a dodge. There are two questions here: First, does not this continuance of an undesirable tradition depend upon overlooking all of the actual complex phenomena which, for example, go to make up civilization? On the other hand, is not mechanism a metaphysical, rather than a scientific, doctrine?

How powerful is the belief that the brain is the bodily seat of

psychological phenomena is illustrated by Stratton's (**104**) assertion in another chapter of the book on Human Error that one general assumption of the phrenologists—namely, the superficial parts of the brain are psychically most significant—is correct. Stratton refers to psychological phenomena as powers. Another illustration of the strength of the cortical or brain dogma is found in the interpretation of cortical losses as the removal of the basis for learning, instead of considering that extirpating experiments indicate that brain functions constitute participating actions in learning processes. Were it not for the force of tradition it would seem strange that extirpation data should not be more impressive in creating doubt concerning brain power and trace theories. The conception of the brain as the seat of consciousness, whether or not translated as the seat of control, dies hard. Extirpation workers appear to be surprised that seriously mutilated organisms display inferior performances.

Generally speaking, it is to be expected that physiologists more than psychologists should perpetuate ideas of neural bases or loci for psychic functions. The former work on the premise that organic processes mediate psychic processes, while the latter are much more committed to the study of interbehavior of organisms with things. In a sense, then, it is not unusual that a physiologist (**5**) can chide psychologists for being unable to distinguish between emotional *behavior* and emotional *experience*. But, since the tradition that psychological phenomena are dependent upon physiological mechanisms is so pervasive, we may expect the thalamo-cortical theory of psychic emotions of Cannon (**18**, **19**), Bard (**5**), and others (**26**) to exert a powerful influence upon psychologists in spite of occasional criticisms (**66**). On the other hand, another line of thought is revealed in the surgeon's suggestion of the neglect and minimization (**2**) of such striking clinical findings as those of Dandy and others.

Dandy (**27**), it will be recalled, makes the following claims:

(1) It is possible to remove all of the right cerebral hemisphere above the basal ganglia with no appreciable disturbance of mentality.

(2) On one occasion both frontal lobes have been completely extirpated for the treatment of a bilateral frontal brain tumor, and following this there has been no appreciable disturbance of mentality. The patient is perfectly oriented as to time, place and person; the memory is unimpaired; he reads, writes and conducts mathematical tests accurately; his conversation is seemingly perfectly normal.

(3) Furthermore, by the excision in other cases of the left occipital

lobe and of the lower third of the left temporal lobe, we can be sure that none of these regions are responsible for intelligence. The intellect, therefore, is concerned with the remaining portion of the left cerebral hemisphere and is doubtless closely related to the speech mechanism.

(4) It has been found after ligation of the anterior cerebral artery on the left side that consciousness is completely and forever lost. This does not result when the same vessel is ligated on the right side. There is, therefore, within the limits of the distribution of this vessel an area specifically concerned with consciousness. The same result follows when this vessel is occluded at the middle of the corpus callosum, indicating that the seat of consciousness is posterior to this point in the vessel.

(5) The entire body of the corpus callosum may be split in the mid-line without any appreciable disturbance of function. This structure is, therefore, eliminated from participation in the important functions which hitherto have been ascribed to it.

Alford (**2**), after reviewing the findings of Dandy (**27**, **28**, **29**, **30**), Gardiner (**34**), O'Brien (**89**), and a number of studies of his own, comes to far-reaching conclusions. First, he regards it as established that Dandy is mistaken in asserting that the ligation of the anterior cerebral artery causes complete and permanent loss of consciousness. His own view is that "a quite small area lying posterially near the base of the brain" on the left side is responsible for "clouding" of consciousness, "confusion," and "dementia." Alford regards his findings as implying that scientists have placed too great reliance on the cortex in connection with mental processes and related functions.

A serious question: Why are there so few references to the clinical findings on human subjects by psychologists (**31**, **101**, **105**)? More interesting still are the asserted implications of such findings for psychological theory and especially the attempt to explain them traditionally. This is significantly true of Squires (**100**), who himself reports an interesting case. That the surgeons who have discovered these phenomena still think in terms of localization of mental functions is obviously nothing but an intellectual lag. As we have seen, Alford, who eliminates the cortex, still regards the basal structures of the brain as the locus. It is indeed unfortunate that psychological phenomena are still looked upon as general functions requiring some sort of neural substratum. This, no doubt, is only an exemplary illustration of the weighty influence of tradition upon our thinking.

Powerful as neural dogmas are, distinctive objections to established neural theory are not lacking. In this connection Skinner's (**98**) advocacy of the study of psychological phenomena without

resort to explanatory neural mechanisms is significant. He reiterates a warning already sounded by Lewes (**67**) and Kantor (**49**, **51**) against imaginary neurology and the attempt to transform the central nervous system into a conceptual nervous system as illustrated by Holt (**43**) and Pratt (**92**). There is still hope, then, that some effect may result from such clear statements as those of Adrian (**1**) and Lashley (**64**, **65**) that we know far more about the behavior we are trying to explain by the nervous system than about the nervous system used as an explanatory mechanism.

Further signs of a possible shuffling off the nervous system coil may be found in McGeoch's (**78**) pointing out "the wisdom of foregoing the speculative delights of the nervous system," in Dunlap's (**32**) discussion of brain functions, in the study of the brain as an organ by the Werthams (**116**), and even in such a book as Rosett's (**96**). The value of the last-mentioned work lies, of course, in its ***absurdum*** character, inasmuch as the author, bravely discussing general and clinical conditions of nervous mechanisms on such a basis as "the cerebral cortex being a storehouse of memories," must add also: "The manner in which experiences are recorded in nerves is so far unknown" (p. 112). This type of book may well become a prelude to the awakening of neurologists and psychologists to the actual place of the nervous system in psychological phenomena. A possible next step may then be taken in the direction of realizing that satisfactory and valid psychological explanations can be achieved in terms of the observed interbehavior between organisms and objects.

Another hopeful outlook for an improved attitude toward neural functions in psychology is found in Krechevsky's (**63**) suggestion that a possible effect of cerebral lesions results in a general diminution of the organism's efficiency. This suggests that an operated animal, because of the mutilation, becomes less effective than a normal one. Another valuable suggestion by the same writer on the basis of his extensive knowledge and expert participation in brain extirpation experiments is the consideration that the comparison of normal and operated animals involves qualitative differences in the situations. As Krechevsky says: "One and the same objective learning problem does not necessarily present the same problem situation to the two animals." In other words, there is a strong presumption here that the neural conditions must be regarded as participating factors in a complex situation. When an animal is operated upon, then a change is brought about in a

complex field situation. Here we have a turning away from the notion that what the animal does is in any sense determined by his brain, which by mutilation has its function partially removed.

IX. Conclusion

In closing this review of illustrative literature bearing on certain trends in psychology I doubt whether the current psychological situation calls for an extreme optimism. My survey of the field indicates that, on the whole, events are not so influential in shaping theory as are cultural patterns. This does not mean that there is a definite lack of progress, since it is not unknown in science that the very grip of tradition itself leads to scientific advancement. A classic example is the development of non-Euclidean geometry because of the firm belief in the absolute nature of Euclidean geometry. Thus, Saccheri, by attempting to prove Euclid's fifth postulate on the assumption that it would be a *reductio ad absurdum* to question it, developed a set of non-Euclidean theorems without even knowing it (**120**). Again, Lobatchevsky, beginning with the same awe of Euclid, finally produced a geometry which, constituting a contradiction of, and a challenge to, a Euclidean axiom, could only bring himself to call it Imaginary. Yet Bell (**8**) names him the Copernicus of geometry.

Because accidental victories over culture are uncontrollable, it is wise to be aware of its influence upon us. In psychology this is easy, for clearly psychology has never shaken off its connection with idealistic philosophy. In this sense the dread of philosophy of many psychologists is well warranted, since an understanding of what their basic postulates imply would mean a necessary shifting of position. Though a genuine operational view would help to rid psychologists of their dualism, this would entail an enormous modification of experimental hypotheses and procedures.

A word of caution in the use of the term "culture." This term is most often employed to refer to expansive ethnic influences upon workers. When we are interested, however, in the relative influence of cultural patterns and investigative conditions upon a particular scientist, culture must be interpreted as the specific and local conditions which operate upon him and exert an effect upon his thinking and research. For example, a functional department or school leads to an overevaluation of learning and underevaluation of sensation research, while a structural laboratory influences the belief in the supreme importance of sensation-investigation.

Again, feeling experiments in Titchener's laboratory do not show mixture, while in Munsterberg's laboratory they do. It is only through such immediate contacts (the influence of a school to which he belongs or being environed by an intellectual tradition) that the larger and more pervasive traditions operate upon a particular scientist. Probably the only safeguard against cultural influences is a powerful faith in events and a determination to understand traditional interferences with our investigation of them.

BIBLIOGRAPHY

1. Adrian, E. G. The nervous system. *Science*, 1936, **84**, 275–278.
2. Alford, L. B. Defects of intelligence from lesions within the central part of the left cerebral hemisphere. *Amer. J. Psychiat.*, 1937, **94**, 615–638.
3. Aliotta, A. The idealistic reaction against science. London: Macmillan, 1914.
4. Anastasi, A. Faculties *versus* factors: a reply to Professor Thurstone. *Psychol. Bull.*, 1938, **35**, 391–395.
5. Bard, P. Emotion: I. The neuro-humoral basis of emotional reactions. *In* Murchison, C. (Ed.), *A Handbook of General Experimental Psychology*. Worcester: Clark Univ. Press, 1934. Pp. 264–311.
6. Bartlett, F. C. Experimental method in psychology. *J. gen. Psychol.*, 1930, **4**, 49–66.
7. Bartlett, F. C. Remembering: a study in experimental and social psychology. Cambridge, Eng.: Cambridge Univ. Press, 1932.
8. Bell, E. T. Men of mathematics. New York: Simon & Schuster, 1937.
9. Bentley, A. F. Linguistic analysis of mathematics. Bloomington: Principia Press, 1932.
10. Bills, A. G. Changing views of psychology as science. *Psychol. Rev.*, 1938, **45**, 377–394.
11. Boring, E. G. Titchener on meaning. *Psychol. Rev.*, 1938, **45**, 92–95.
12. Bridgman, P. W. The logic of modern physics. New York: Macmillan, 1927.
13. Bridgman, P. W. Operational analysis. *Phil. Sci.*, 1938, **5**, 114–131.
14. Brown, J. F. A methodological consideration of the problem of psychometrics. *Erkenntnis*, 1934, **4**, 46–61.
15. Brown, J. F. Freud and the scientific method. *Phil. Sci.*, 1934, **1**, 323–337.
16. Brown, J. F. On the use of mathematics in psychological theory. *Psychometrika*, 1936, **1**, I, 77–90; **1**, II, 7–15.
17. Brown, J. F. Psychology and the social order. New York: McGraw-Hill, 1936.
18. Cannon, W. B. The James-Lange theory of emotions: a critical examination and an alternative theory. *Amer. J. Psychol.*, 1927, **39**, 106–124.
19. Cannon, W. B. Again the James-Lange and the thalamic theories of emotion. *Psychol. Rev.*, 1931, **38**, 281–295.
20. Carnap, R. The unity of science. London: Kegan Paul, 1934.
21. Carnap, R. Philosophy and logical syntax. London: Kegan Paul, 1935.
22. Carr, H. The quest for constants. *Psychol. Rev.*, 1933, **40**, 514–532.
23. Child, C. M. Physiological foundations of behavior. New York: Holt, 1924.

24. Crozier, W. J. The study of living organisms. *In* Murchison, C. (Ed.), *Foundations of Experimental Psychology.* Worcester: Clark Univ. Press, 1929. Pp. 45–127.

25. Crozier, W. J., & Hoagland, H. The study of living organisms. *In* Murchison, C. (Ed.), *A Handbook of General Experimental Psychology.* Worcester: Clark Univ. Press, 1934. Pp. 3–108.

26. Dana, C. L. The anatomic seat of the emotions: a discussion of the James-Lange theory. *Arch. Neurol. Psychiat., Chicago,* 1921, **6**, 634–639.

27. Dandy, W. E. Changes in our conceptions of localization of certain functions of the brain. *Amer. J. Physiol.*, 1930, **93**, 643.

28. Dandy, W. E. The effects of total removal of the left temporal lobe in a right-handed individual: localization of areas of the brain concerned in speech. *J. nerv. ment. Dis.*, 1931, **74**, 739–742.

29. Dandy, W. E. The brain. *In* Lewis, D. (Ed.), *Practice of Surgery.* Hagerstown: Prior, 1932. Vol. 12, pp. 1–682.

30. Dandy, W. E. Physiological studies following extirpation of the right cerebral hemisphere in man. *Johns Hopk. Hosp. Bull.*, 1933, **53**, 31–51.

31. Diven, K. Dandy's radical extirpations of brain tissue in man. *Amer. J. Psychol.*, 1934, **46**, 500–503.

32. Dunlap, K. Psychological hypotheses concerning the functions of the brain. *Sci. Mon., N. Y.*, 1930, **31**, 97–112.

33. Eaton, R. M. General logic. New York: Scribner, 1931.

34. Gardiner, W. J. Removal of the right cerebral hemisphere for infiltrating glioma. *J. Amer. med. Ass.*, 1933, **101**, 823–826.

35. Garrett, H. E. Lewin's 'topological' psychology: an evaluation. *Psychol. Rev.*, 1939, **46**, 517–524.

36. Goldstein, K. The organism. New York: American Book, 1939.

37. Gurwitsch, A. Über den Begriff des embryonalen Feldes. *Arch. EntwMech. Org.*, 1922, **51**, 383–415.

38. Gurwitsch, A. Weiterbildung und Verallgemeinerung des Feldbegriffs. *Roux's Arch.*, 1927, **112**, 433–454.

39. Hecht, S. Vision: II. The nature of the photoreceptor process. *In* Murchison, C. (Ed.), *A Handbook of General Experimental Psychology.* Worcester: Clark Univ. Press, 1934. Pp. 704–828.

40. Helmholtz, H. von. Counting and measuring. New York: Van Nostrand, 1930.

41. Herrick, C. J. Neurological foundations of animal behavior. New York: Holt, 1924.

42. Herrick, C. J. Error in neurophysiology. *In* Jastrow, J. (Ed.), *The Story of Human Error.* New York: Appleton-Century, 1936. Pp. 251–267.

43. Holt, E. B. Animal drive and the learning process. New York: Holt, 1931.

44. Hull, C. L. The conflicting psychologies of learning: a way out. *Psychol. Rev.*, 1935, **42**, 491–516.

45. Hull, C. L. Mind, mechanism and adaptive behavior. *Psychol. Rev.*, 1937, **44**, 1–32.

46. Jastrow, J. (Ed.) The story of human error. New York: Appleton-Century, 1936.

47. Johnson, H. M. Some follies of "emancipated" psychology. *Psychol. Rev.*, 1932, **39**, 293–323.

48. Johnson, H. M. Rival principles of causal explanation in psychology. *Psychol. Rev.*, 1939, **46**, 493–516.
49. Kantor, J. R. The nervous system: psychological fact or fiction. *J. Phil.*, 1922, **19**, 38–49.
50. Kantor, J. R. Can the psychophysical experiment reconcile introspectivists and objectivists? *Amer. J. Psychol.*, 1922, **33**, 481–510.
51. Kantor, J. R. The organismic versus the mentalistic attitude towards the nervous system. *Psychol. Bull.*, 1923, **12**, 684–692.
52. Kantor, J. R. Concerning physical analogies in psychology. *Amer. J. Psychol.*, 1936, **48**, 153–164.
53. Kantor, J. R. The nature of psychology as a natural science. *Acta psychol.*, 1938, **4**, 1–61.
54. Kantor, J. R. The operational principle in the physical and psychological sciences. *Psychol. Rec.*, 1938, **2**, 1–32.
55. Kantor, J. R. Postulates for a logic of specificity. *J. Phil.*, 1940, **37**, 29–42.
56. Kelly, G. A. The assumption of an originally homogeneous universe and some of its statistical implications. *J. Psychol.*, 1938, **5**, 201–208.
57. Kent, G. H. Use and abuse of mental tests in clinical diagnosis. *Psychol. Rec.*, 1938, **2**, 391–400.
58. Koffka, K. The principles of gestalt psychology. New York: Harcourt, Brace, 1935.
59. Köhler, W. Die physischen Gestalten in Ruhe und im stationären Zustand. Erlangen: Verlag der philosophen Akademie, 1924.
60. Köhler, W. The mentality of apes. New York: Harcourt, Brace, 1925.
61. Köhler, W. The place of value in a world of facts. New York: Liveright, 1938.
62. Korzybski, A. Science and sanity. Lancaster: Science Press, 1933.
63. Krechevsky, I. Brain mechanisms and brightness discrimination learning. *J. comp. Psychol.*, 1936, **21**, 405–441.
64. Lashley, K. S. Basic neural mechanisms in behavior. *Psychol. Rev.*, 1930, **37**, 1–24.
65. Lashley, K. S. Learning: III. Nervous mechanisms in learning. *In* Murchison, C. (Ed.), *A Handbook of General Experimental Psychology.* Worcester: Clark Univ. Press, 1934. Pp. 456–496.
66. Lashley, K. S. The thalamus and emotion. *Psychol. Rev.*, 1938, **45**, 42–61.
67. Lewes, G. H. The physical basis of mind. London: Kegan Paul, 1893.
68. Lewin, K. A dynamic theory of personality. New York: McGraw-Hill, 1935.
69. Lewin, K. Principles of topological psychology. New York: McGraw-Hill, 1936.
70. Lewin, K. The conceptual representation and the measurement of psychological forces. In *Contributions to Psychological Theory.* Durham: Duke Univ. Press, 1938. Vol. I, No. 4.
71. Lewin, K. Reply to Dr. Garrett. *Psychol. Rev.*, 1939, **46**, 591–594.
72. Lewis, C. I. A survey of symbolic logic. Berkeley: Univ. California Press, 1918.
73. Lewis, G. N. The anatomy of science. New Haven: Yale Univ. Press, 1926.
74. Loeb, J. The organism as a whole. New York: Putnam, 1916.
75. Loeb, J. Tropisms, forced movements and animal conduct. Philadelphia: Lippincott, 1918.

76. MACH, E. The science of mechanics: a critical and historical account of its development. Chicago: Open Court, 1907.
77. MAST, S. O. Light and the behavior of organisms. New York: Wylie, 1911.
78. MCGEOCH, J. A. Forgetting and the law of disuse. *Psychol. Rev.*, 1932, **39**, 352–370.
79. MCGEOCH, J. A. A critique of operational definition. (Abstract.) *Psychol. Bull.*, 1937, **34**, 703–704.
80. MELTON, A. W. The methodology of experimental studies of human learning and retention. *Psychol. Bull.*, 1936, **33**, 305–394.
81. METZGER, W. Gesetze des Sehens. Frankfort am Main: Kramer, 1936.
82. MILLER, J. G. Symbolic technique in psychological theory. *Psychol. Rev.*, 1939, **46**, 464–479.
83. MURCHISON, C. (Ed.) A handbook of child psychology. Worcester: Clark Univ. Press, 1933.
84. MURCHISON, C. (Ed.) A handbook of general experimental psychology. Worcester: Clark Univ. Press, 1934.
85. MURCHISON, C. (Ed.) A handbook of social psychology. Worcester: Clark Univ. Press, 1935.
86. MURPHY, G., MURPHY, L. B., & NEWCOMB, T. N. Experimental social psychology. New York: Harper, 1937.
87. MURRAY, H. A. Explorations in personality: a clinical and experimental study of 50 men of college age. New York: Oxford Univ. Press, 1938.
88. MYERS, C. S. Psychological cautions in the use of statistics. *Z. angew. Psychol.*, 1930, **36**, 82–86.
89. O'BRIEN, J. D. Further report on case of removal of the right cerebral hemisphere. *J. Amer. med. Ass.*, 1936, **107**, 657–659.
90. OGDEN, C. K., & RICHARDS, I. A. The meaning of meaning: a study of the influence of language upon thought and of the science of symbolism. New York: Harcourt, Brace, 1923.
91. PEATMAN, J. G. On the meaning of a test score in psychological measurement. *Amer. J. Orthopsychiat.*, 1939, **9**, 23–46.
92. PRATT, C. C. The logic of modern psychology. New York: Macmillan, 1939.
93. QUINE, W. V. Truth by convention. In *Philosophical Essays for Alfred North Whitehead.* New York: Longmans, Green, 1936. P. 90.
94. RASHEVSKY, N. Mathematical biophysics and psychology. *Psychometrika*, 1936, **1**, 1–26.
95. RITTER, W. E. The unity of the organism. Boston: Badger, 1919.
96. ROSETT, J. The mechanism of thought, imagery, and hallucination. New York: Columbia Univ. Press, 1939.
97. SKINNER, B. F. The concept of the reflex in the description of behavior. *J. gen. Psychol.*, 1931, **5**, 427–458.
98. SKINNER, B. F. The behavior of organisms: an experimental analysis. New York: Appleton-Century, 1938.
99. SPEMANN, H. Embryonic development and induction. New Haven: Yale Univ. Press, 1938.
100. SQUIRES, P. C. An interesting case of human behavior after a loss of brain tissue. *J. gen. Psychol.*, 1932, **6**, 206–209.
101. SQUIRES, P. C. The problem of auditory bilateral cortical representation with special reference to Dandy's findings. *J. gen. Psychol.*, 1935, **12**, 182–193.

102. STEVENS, S. S. Psychology and the science of science. *Psychol. Bull.*, 1939, **36**, 221–263.

103. STEVENS, S. S., & DAVIS, H. Hearing: its psychology and physiology. New York: Wiley, 1938.

104. STRATTON, G. M. Error in psychology. *In* Jastrow, J. (Ed.), *The Story of Human Error*. New York: Appleton-Century, 1936. Pp. 322–344.

105. THOMPSON, W. H. Brain surgery and psychology. *J. abnorm. soc. Psychol.*, 1936, **31**, 285–290.

106. THURSTONE, L. L. Current misuse of the factoral methods. *Psychometrika*, 1937, **2**, 73–76.

107. THURSTONE, L. L. Shifty and mathematical components. *Psychol. Bull.*, 1938, **35**, 223–236.

108. TRYON, R. C. So-called group factors as determiners of abilities. *Psychol. Rev.*, 1932, **39**, 403–439.

109. TRYON, R. C. A theory of psychological components: an alternative to mathematical factors. *Psychol. Rev.*, 1935, **42**, 425–454.

110. WEISS, A. P. A theoretical basis of human behavior. Columbus: Adams, 1925.

111. WEISS, P. Ganzregenerate aus halben Extremitätenquerschnitt. *Roux's Arch.*, 1926, **107**, 1–53.

112. WEISS, P. Principles of development. Chicago: Univ. Chicago Press, 1939.

113. WELLMAN, B. L. Some new bases for interpretation of the IQ. *J. genet. Psychol.*, 1932, **41**, 116–126.

114. WELLMAN, B. L. Our changing concept of intelligence. *J. consult. Psychol.*, 1938, **2**, 97–107.

115. WELLMAN, B. L. The IQ: a reply. *J. Psychol.*, 1939, **8**, 143–155.

116. WERTHAM, F., & F. The brain as an organ: its postmortem study and interpretation. New York: Macmillan, 1934.

117. WILLIAMS, R. D. Studies in contemporary psychological theory: I. What is behavior space? *J. Psychol.*, 1938, **6**, 69–79.

118. WOODGER, J. H. The axiomatic method in biology. Cambridge, Eng.: Cambridge Univ. Press, 1937.

119. WOODWORTH, R. S. Experimental psychology. New York: Holt, 1938.

120. YOUNG, J. W. Lectures on fundamental concepts of algebra and geometry. New York: Macmillan, 1916.

BOOKS AND MATERIALS RECEIVED

Betts, E. A., & Ayers, A. W. Data on visual sensation and perception tests. Part III: Stereopsis. Meadville, Pa.: Keystone View, 1940. Pp. 37.

Brown, J. F., with the collaboration of K. A. Menninger. The psychodynamics of abnormal behavior. New York & London: McGraw-Hill, 1940. Pp. xvi+484.

Bryant, M. M., & Aiken, J. R. Psychology of English. New York: Columbia Univ. Press, 1940. Pp. 229.

Coleman, A., with the coöperation of C. B. King (Compilers). English teaching in the Southwest: organization and materials for instructing Spanish-speaking children. Washington, D. C.: American Council on Education, 1940. Pp. xvii+307.

Fries, C. C., with the coöperation of A. A. Traver. English word lists: a study of their adaptability for instruction. Washington, D. C.: American Council on Education, 1940. Pp. ix+109.

Gallup, G., & Rae, S. F. The pulse of democracy: the public opinion poll and how it works. New York: Simon & Schuster, 1940. Pp. x+335.

Hanes, G. M., & Benz, H. E. Hanes-Benz biology test (Forms A and B); Key; Manual of directions; *and* Class record sheet. Cincinnati: Gregory, 1939. Pp. 8.

Pond, F. L. Inventory of reading experiences *and* Manual of directions. Stanford Univ.: Stanford Univ. Press, 1940. Pp. 7; 7.

Rexroad, C. N. Psychology and personality development. Boston: Christopher, 1940. Pp. 501.

NOTES AND NEWS

M. EDOUARD CLAPARÈDE, professor of psychology at the University of Geneva and permanent secretary of the International Congress of Psychology, died on September 2. During recent years he had specialized in child psychology and had established the J. J. Rousseau Corresponding Institute of France.—*Science.*

THE Washington-Baltimore branch of the American Psychological Association held its first meeting of the academic year at the George Washington University, Washington, D. C., on October 31, 1940. Papers were presented by: Dr. Willard Harrell, University of Illinois; Mr. William Shanner, War Department; and Dr. Marion Richardson, U. S. Civil Service Commission.

IT has been announced that the National Institute of Health of the United States Public Health Service is organizing a new unit for research into some of the many problems of aging. These problems are divisible into three major fields of investigation: (1) the biology of senescence as a process, (2) the human clinical problems of aging and of diseases characteristically associated with advancing years, which include the mental changes of senescence as well as the physical changes, and (3) the socioeconomic problems of a shifting age distribution in the population. The Institute is concerned with the first two of these divisions.

The first service to scientific research which the Unit on Gerontology is undertaking is to conduct a survey of the present trends of active and contemplated investigations into the problems of aging in American scientific institutions. The survey is intended to ascertain what problems are being studied and what methods of approach are being applied. In addition to these studies, many investigations which do not pertain directly to aging should yield data useful to workers in gerontology. The Unit is especially interested in knowing of these indirectly related studies, the full implications of which are far too often obscured in their published titles.

From the clinical viewpoint, the Unit's greatest concern is with those studies dealing with health evaluation, mensuration of functional capacity (including criteria of "physiologic age") and with those diseases whose incidence increases sharply in later life (the so-called "degenerative disorders").

In view of the great, but largely latent and scattered, interest in the problems of aging, it is the hope of the Unit that the present survey may aid in the promotion of closer coöperation of the scientists interested in these fields.

Information concerning subjects under investigation and the methods of approach is earnestly solicited. Letters should be addressed to Dr. Edward J. Stieglitz, in charge of Investigations in Gerontology, National Institute of Health, U. S. Public Health Service, Bethesda, Md.

NOTES AND NEWS

Dr. H. [illegible] professor of psychology at the University of [illegible]

The Washington [illegible] Association held its first meeting [illegible]

It has been [illegible]

[illegible]

From the [illegible] point, the [illegible]

In view of the [illegible]

Information concerning [illegible] Letters should be addressed to Dr. [illegible] of Health, U. S. Public Health Service, Bethesda, [illegible]